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NATURAL SCIENCE

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Specific Characters in Bacteria

In the April number of the *Scottish Medical and Surgical Journal* Dr Alexander Johnson records two cases of puerperal fever successfully treated by the antistreptococcus serum. There are many similar cases recorded, and there are also many in which serum treatment has been unavailing. The matter is not merely one of medical importance, but involves a problem of no small interest to the biologist—the question, namely, of specificity amongst bacteria. Even among the higher plants and animals this question is not rarely a matter of dispute, although as a rule, morphological characters are alone at issue. It is not surprising therefore, that amongst bacteria,—where morphology alone is of little value as a means of specific distinction, though it is useful enough in differentiating genera—the difficulties which arise should be even more acute. In distinguishing between allied species, bacteriologists employ, besides morphological characters, staining reactions, cultural characters, chemical and physiological properties, and powers of pathogenesis. To these aids to diagnosis there has been added, in the last few years, an altogether novel one—the capacity for specific immunisation, together with the remarkable power possessed by the serum of immunised animals, in the case of certain bacteria, of agglutinating the bacteria against which they have been immunised. Thus, to take a concrete example, supposing that it be desired to distinguish the bacillus of typhoid fever from one of its nearest allies, the common colon bacillus, the bacteriologist can rely largely upon morphological distinctions, and in particular upon the character and number of the cilia demonstrable by the methods of Löffler, Pitfield, or Van Ermengem. He can trust also to chemical tests—to the powers possessed by the colon bacillus of coagulating milk in virtue of its more active acid production, of its rich powers of gas formation, or of indol production—powers which the typhoid bacillus does not possess. But he can now adopt a new method. He can immunise an animal, by repeated injection of sub-fatal doses, against the colon bacillus or the typhoid bacillus, and he can test the power possessed by the serum of such immune animals of agglutinating the bacillus which he wishes to test. Serum from an animal immunised

against the colon bacillus possesses no agglutinating power upon the typhoid bacillus, and *vice versâ*. This test is probably of more value in distinguishing between the two species than any other one test, except, perhaps, the number of cilia. Yet even here the distinction is not absolute. Durham has shown that there is a bacillus, the *B. enteritidis* of Gärtner (intermediate in character between the typhoid bacillus and *B. coli communis*, though probably to be regarded as a variety of the latter), which is feebly agglutinated by typhoid serum, although not in such high degrees of dilution as is the typhoid bacillus itself.

There are some species of bacteria which are sharply marked off: thus the tetanus bacillus is one which both morphologically and in its pathogenic powers is a distinct and definite species. The typhoid bacillus and *B. coli communis* are members of a group in which the reverse is the case. The epidemiology of typhoid fever can leave no doubt on the mind that *B. typhosus* is a distinct and fixed species; yet apart from the disease it produces, its certain recognition is, as above indicated, not always so easy a matter. The colon bacillus occurs in countless varieties, and has been described under many different names. Its specific characters are ill-marked, and it is probably a rapidly varying dominant species, the characters of which are not yet fixed.

The same may be said of the group of *Streptococci*. In the field of pathogenesis they are as it were a 'dominant' group, with varying and ill-defined specific characters, and with equally varying and ill-defined pathogenic effects. Unlike *B. typhosus*, which causes one distinct and definite disease, it seems probable that a single species of *Streptococcus* may give rise to suppuration, erysipelas, malignant endocarditis, puerperal fever, septic peritonitis, and half a dozen other diseases which clinically are distinct enough. And there are probably a number of different species of *Streptococci*, any one of which may cause a number of different diseases according to its grade of virulence and seat of infection, while clinically similar diseases may be due at different times to different species of *Streptococcus*. Specificity, if such there be, is here at its very vaguest.

Compared with tetanus and diphtheria, two well-marked species causing definite disease, streptococcus infection is a most complex and ill-defined condition, and the task of the bacteriologist in providing an antistreptococcus serum is proportionately difficult. No one can venture to affirm with confidence how many pathogenic species of *Streptococcus* exist, nor whether a given case of disease is due to one or other of the supposed species which are recognised. Marmorek, in preparing his antistreptococcus serum, employed a *Streptococcus* which he obtained from the fauces, and the virulence of

which he increased enormously by cultivating it in the peritoneal cavities of a series of rabbits. Other serums have been prepared from other sources by different workers. But antitoxic action is subject to the laws of specificity to the same extent as toxic action. A given antitoxic serum will immunise against, or cure the disease produced by, just that one species of micro-organism which was employed in producing the serum. In the case of *Streptococcus* infection, it is therefore not remarkable that while in some cases a given serum will produce most striking curative results, in others it is absolutely powerless. In the present state of knowledge it is not possible to foresee which case will benefit and which will not. But the fact that such differences exist may serve as a warning against the supposed unity of certain species of *Streptococcus*, maintained by some observers.

The Effects of Tropical Climate

The exploration and first attempts at the administration of Africa have been attended with so serious a loss of life from disease, that it is not surprising that those interested in Africa should sometimes despair of its ultimate success. They throw the blame on that most indefinite of factors, the climate, and attempts to discuss tropical sanitation only too often degenerate into mere denunciation of that scapegoat. The afternoon meeting of the Royal Geographical Society, which assembled on April 27th to hear Dr Sambon's paper on the possibilities of the acclimatisation of the white races in tropical regions, was no exception to the rule. Dr Sambon stoutly held that there is no reason why whites should not live and thrive in the tropical zone as well as they do in the temperate zones; but the meeting, in spite of Dr Manson's powerful support of Dr Sambon's propositions, would not be comforted. The discussion was interesting, as it could not fail to be when such authorities as Dr Manson, Sir John Kirk, Sir Harry Johnston, and Mr J. A. Baines took part in it. But the discussion was disappointing as well, for the pessimists did not join issue on the material point. They denounced the climate, even in places where it is described as "appearing delightful," and they pointed to past experience, as told by the mournful death roll or the degeneration of European races, such as the Spaniards in South America and the Portuguese in East Africa. But no one denies either the deaths or the degeneration. The question is whether they are due to unalterable factors of climate, or to organic diseases which may be met and defeated. Dr Sambon denied the climatic theory, and went through the climatic factors one by one, and showed that they alone are not injurious to health. He challenged those who hold that it is the climate which does the mischief to tell him how it acted, through what elements, and what

organic injury it causes. But not a word of explanation on these points was given by his opponents. Dr Sambon attributed the mortality of the tropics to three diseases—dysentery, haematinuria, and malaria. These are, undoubtedly, due to specific organisms which are especially prevalent in the tropical zone, but are not necessarily connected with the heat. If the existing white mortality and the past degeneration of races long subjected to the effects of these maladies are simply due to them, then there is good hope for the future. Dr Manson cited the case of that most repulsive of diseases, elephantiasis, which was once one of the terrors of the tropics, and was then charged to the climate. But Manson has shown that elephantiasis is due to the organism known as *Filaria;* he has worked out the life-history of the parasite, and shown how it enters the human body. People being thus warned have only themselves to thank if they now contract elephantiasis. This case gives us good hope that, when the life-histories of the haematozoa of malaria, dysentery, and haematinuria have been similarly worked out, the diseases will be brought similarly under control. That is the hope for the future, and what is wanted is more knowledge of the biology of the parasites. Malaria is now being admirably studied by the medical schools of Rome and Vienna. Haematinuria is the most obscure and deadly of African diseases, and it will continue to entail on England lamentable sacrifices of life and money until it can be dismissed as Manson has dismissed the fear of elephantiasis. Most of the doctors who find their way to our African tropical protectorates are medical sportsmen and not medical biologists. They probably could not focus a high-power microscope if they tried. An institute for the study of tropical diseases is urgently needed, and would pay as a policy of imperial insurance.

For the Lady Cyclist

In the June number of the *Scottish Medical and Surgical Journal*, Dr J. W. Ballantyne gives a valuable digest of forty-five papers that have been written on bicycling for women. On the whole it appears that the advantages, from a physiological point of view, wholly outweigh the disadvantages, if these be guarded against by proper precautions. These latter are chiefly associated with the choice of a machine, the essential point being that the seat should be suitably placed and adapted to the anatomy of the female pelvis. "It should be pretty well forward, and when the cyclist is erect in the saddle her heels should touch the pedal when lowest, her feet being in the horizontal position. The commonest faulty position is having the saddle too low and too far back; on the other hand, the saddle too high is also wrong, causing over-stretching of the knee and ankle,

which is very tiring; a perpendicular dropped from the hip should pass through the centre of the pedal, and with the feet at the lowest point the knee should be slightly bent. Most saddles have been made too narrow, the cyclist thus being compelled to ride on the perineum instead of the ischial tuberosities, and in many instances the pommel or peak has been too high." It is obvious to any cyclist that all these points refer with equal force to the male sex. So also does the advice that the cyclist should not ride to the point of exhaustion, should not have the gearing too high or the machine too heavy, and should ride in a suitable dress. With regard to the last-mentioned point the only question is, what is suitable? Since Dr Ballantyne is a man, it is unlikely that he has ever ridden "in a shortened skirt, with modified corset," until he has attempted this, especially in a wind, we cannot consider his advice on the matter of the smallest value. Many of the points in the paper are of considerable interest, but hardly to be dealt with in the pages of this Review. We can, however, strongly recommend it to any who may have thrown upon them the professional duty of advising lady cyclists.

The Australian Museum

The Report of this Museum for 1896, which we received a short time ago, receives considerable interest from the out-spoken remarks of the curator, Mr R. Etheridge, junior. For one thing, Mr Etheridge complains justly and forcibly of the inadequate scale of remuneration received by the staff individually in comparison with that prevailing in some of the service departments; although, as he points out, the scientific assistants are, by educational status and scientific attainments, entitled to rank as professional men. What applies to the assistants applies also to the mechanics, whose work is undoubtedly of a skilled and special character. Even the attendants of a scientific museum are put off with less pay than those of an art gallery. Not only is this the case, but the Museum remains much undermanned. Of course all museums are undermanned, just as in most countries museum assistants are underpaid; but certainly the Government of New South Wales asks a little too much when it expects even a person of such energy as Mr Etheridge to combine the functions of curator and those of sole palaeontologist. Mr Etheridge says, and most people will agree with him, "I regard the position of curator of such an institution as this as one carrying with it the necessity of engaging in original research. As matters are at present constituted this is an impossibility."

Among the difficulties under which our Australian colleagues labour, not the least is the destruction constantly effected by the white ants. We have already alluded to the ravages committed by them in the Australian Museum, but it appears that these were

even worse than was at first supposed. Not only had the roof to be renewed, but the flooring and joists of the main hall were found to be burrowed by the termites, which had also made their way through the masonry joints into and under the floor of the Ethnological Hall, and had as completely destroyed the woodwork of that structure as of the roof. The remedying of all this naturally led to great expense and to much waste of time in removing and again replacing the whole of the collections. It is satisfactory to find that, in spite of this, work has been begun on a new spirit-room and workshops, although in connection with those as well as with many other matters, Mr Etheridge finds it necessary to note "much unnecessary delay."

To return to the brighter side of affairs. The presentations to the Museum include several items of much interest. Chief is the celebrated 'Dobroyde' collection of Australian birds and eggs brought together by the late curator, Dr E. P. Ramsay, and his brothers at their home in Dobroyde, Ashfield, N.S.W. This collection contains a large number of type-specimens. It was purchased from Mr J. S. Ramsay by the Government of New South Wales and delivered by it to the trustees of the Museum. Mr W. A. Horn has presented further collections from the results of his recent expedition into the interior, and these include further type-specimens. Another valuable donation is a piece of meteoric iron, weighing over 44 lbs. It was found on the Nocoleche holding near Wanaaring, N.S.W., and will be known as the Nocoleche Meteorite. The donor was Mr G. J. Raffel. The meteorite has been cut and polished by Mr H. A. Ward of Rochester, N.Y., and a few slices are available for exchange. Mr C. W. Darley, engineer-in-chief for harbours and rivers, presented the Museum with some fossil remains of a dugong, discovered during the excavations for a canal at Shea's Creek, Alexandria, near Sydney. This is the first instance of the discovery of dugong remains so far south. A noteworthy addition to the collections is the skeleton of the Indian elephant, which, under the name of Jumbo, was a familiar feature of the Sydney Zoological Gardens. This has been satisfactorily set up by Messrs H. Barnes and H. Barnes, junior, but space is not at present available for the mounting the skin. It is most distinctly to be noted that this Jumbo is not the same as the erstwhile ornament of our own 'Zoo' and of Mr Barnum's show.

The whole impression made upon us by this Report is that the staff of the Australian Museum, however undermanned and underpaid it may be, has managed in spite of unprecedented difficulties to accomplish some excellent work from both the scientific and the museum point of view; and it is sincerely to be hoped that the Government of New South Wales may with the return of pros-

perity be able to appropriate larger sums, with greater promptness, to the establishment which, in virtue of its importance, is rightly known as the Australian Museum.

Notes from Singapore

Dr R. Hanitsch, Curator and Librarian of the Raffles Library and Museum, Singapore, has succeeded in obtaining from his Committee a sum of $500, for the purchase of zoological works, which we hope will enable him to continue his zoological studies with greater facility. He has done some collecting on the coral reefs at Blakang Mati, where the most striking forms are numberless Antedonidae (feather-stars). The sea-urchin *Heterocentrotus mammillatus*, has for the first time been obtained in perfect specimens, although the thick spines of it are to be seen by sacks full in the native shops; some say that they are used as the mouth-pieces of pipes, others that they are medicine. It is interesting in this connection to recall the fact that spines of fossil sea-urchins pounded up and drunk with water were used in olden times in Europe as a remedy for stone in the bladder. The Museum has also been presented by Mr Maclear-Ladds with a perfect specimen of *Pentacrinus* (so-called); it is the first ever received by it, and came from the Jahal Bank, ninety miles south of Timor, depth 110 fathoms.

This Museum does not yet contain a typical collection of Malayan fauna, since the majority of specimens collected in that part of the world are sent to Europe and America, while the Curator of the Raffles Museum has succeeded in getting five days for collecting, for the first time for several years. Every museum of importance should have a collector in its own pay, or should give special facilities for collecting to the members of its staff. This is the policy of the leading museums in all countries, except of course our own. Under these circumstances it is pleasant to read that Dr G. D. Haviland has presented the Raffles Museum with a valuable series of ants, including several type-specimens collected by himself in Singapore, Perak, and Sarawak, and identified by Prof. A. Forel of Zurich. Several specimens of reptiles and amphibians have been received in exchange from Lieut. Stanley Flower of Bangkok Museum. The first of the fossiliferous rocks ever obtained from the Peninsula has come from a railway cutting near Kuala Lipis, Pahang, having been presented by Mr H. F. Bellamy, but its age is not hinted at. The skeleton of a large male orang-utan has been mounted, and its dentition has been found to be abnormal, the lower jaw having four well-developed molars on each side. This Museum has now a rival to Aaron's Rod, for a tree trunk against which

one of the orang-utan skeletons is mounted, after having been several months in the case, began suddenly to sprout, and bore green twigs for several months, during which period it proved the chief attraction in the Museum.

We never yet knew a Curator who did not require more room. Needless to say Dr Hanitsch proves no exception.

Lessons from Chicago

We have received the Annual Report of F. J. V. Skiff, the Director of the Field Columbian Museum, Chicago, for 1896-7. The staff of this Museum comprises: G. A. Dorsey, Acting Curator of Anthropology; C. F. Millspaugh, Curator of Botany; O. C. Farrington, Curator, and H. W. Nicholls, Assistant Curator, Department of Geology; D. G. Elliot, Curator, and S. E. Meek, Assistant Curator, Department of Zoology; C. B. Cory, Curator of Ornithology. The Librarian is J. Dieserud, and the Recorder, D. C. Davies. These and others have given numerous lectures on subjects connected with the Museum or with the explorations of its officials. The Museum issued during the year eight publications, of which the most important was "Archaeological Studies among the Ancient Cities of Mexico," by W. H. Holmes. The library is making satisfactory progress; but since the Museum only receives, by purchase or exchange, ninety-two periodicals, it cannot be considered particularly complete in that department. We notice, however, that a list of all the periodicals in all the libraries of Chicago has been prepared, and this no doubt will lead to the co-operation of the numerous institutions in that city.

Among the accessions to this Museum are several hundred Etruscan antiquities of earthenware and bronze, excavated under the direction of Prof. Frothingham in 1895-6; Egyptian antiquities, presented by Prof. Flinders Petrie; ancient pottery from Georgia; a meteorite from Mexico, and specimens from eighteen other meteorites. Among the notable collections obtained by the Botanical Department during the past year are Pringle's Mexican plants, Palmer's Durango collection, Nash's and Pollard's Florida and Mississippi plants, the Sandberg Idaho collection, Gaumer's last Yucatan species, Jenman's British Guiana and Rusby's Orinoco collections, Schlechter's South African species; the complete lichen herbarium of Calkins; and the important personal herbarium of the late Dr Schott, the latter including plants from Yucatan, Panama, and Mexico.

We have not mentioned the numerous collections obtained by D. G. Elliot and the members of his expedition to Somali Land. We have received a special report on the fish they collected, containing descriptions of some of the new and rare species. With reference to this expedition Prof. Elliot writes: "It is the only proper

way to secure collections for a museum," a sentiment that we heartily endorse. There is also contained in this report an account of a collecting trip made by Mr Dorsey and Mr Allen, the photographer of the Museum, among the Indians of the far west. As a consequence of this, it is believed that the Museum now possesses the most complete existing representation of the North-west coast Indians. Our American cousins, advanced as they are in all branches of museum work, naturally understand the importance to a museum of having its own trained collectors, and the urgent need at the present stage of the earth's history of securing specimens of those zoological and ethnological types which may be extinct before many years have passed. Is it not better to invest money in this way, than to waste it on the purchase of ancient collections and unauthenticated dealers' specimens?

An exhibit illustrating the forestry of North America is being prepared by Mr Millspaugh. Each section of the exhibit comprises a glazed and framed tray, containing a branch, flowers and fruits, and a block of wood from the same tree; a photograph of the tree in summer and the same tree in winter, both from the same point of view; a seven-foot trunk and transverse section; a commercial plank; a two-foot map of North America, coloured to show the distribution of the species; and a series of ornamental cabinet specimens of the wood. A detailed account of these exhibits and the method of preparing them is given, and will well repay study by curators. We may also recommend to practical museum-workers the account of the exhibit of metallurgical processes, which is arranged on a somewhat novel plan, showing the various stages of the process by means of lines connecting the specimens.

The report is illustrated by twelve plates, most of them in half-tone. Some of them illustrate practical details, others show some mounted groups. Among the latter we may draw attention to the group of herons and that of the Lesser Koodoo. If our readers inquire how it is that an institution which has none too much money can afford to illustrate its reports in this lavish style, we may explain that the Museum retains the services of a professional photographer, and keeps all the blocks illustrating the publications of the Museum. Most leading museums now have photographers attached to their staff, the exception, as usual, is furnished by our own country.

In conclusion we should like to ask why it is that reports which come to us from American museums are always interesting to read, in strong contrast to the reports which come from most similar establishments in our own country and in Europe. It would seem that the writing of these reports is a labour of love to the Americans, while our own curators only do it as a piece of official routine. The consequence is that, in the present Report, as an example, the

curator finds hints, suggestions, and actual information of value to himself; whereas the Report of, say, the British Museum, contains little but lists of donations and the numbers of specimens registered during the year, with similar matter of no use to anybody in the wide world.

Whales at the British Museum

It is not as though our museums had nothing of general interest to record, nothing of special interest to curators of other museums. At the British Museum (Natural History), for instance, the enlightened administration of Sir William Flower has introduced many novelties, which may be casually alluded to as having occupied the time of such and such assistants or artisans, but which are not explained in the Annual Report. One such interesting and important addition has been completed this very month. No museum has hitherto solved the difficulty of exhibiting the outward form of the various kinds of whales, which baffle the taxidermist's art on account of the oily nature of their skin. At last, however, Sir William Flower has solved the problem in a most satisfactory manner, and the result is a unique addition to the Department of Zoology in the museum over which he presides. The new Gallery of Cetacea was opened to the public for the first time during the Whitsuntide holidays, and the exhibition is no longer a forest of dry bones, but a selection of the principal types of cetacean life displaying not only the skeleton, but also the outward form. Each skeleton is mounted in the ordinary manner on iron supports, and a second frame of more elaborate construction is fixed on one side—the side from which the visitor first sees the specimen. This frame reproduces the original contour of the animal, and is covered with a peculiar composition somewhat similar to *papier maché;* this represents the skin, and is finally painted with a tint and gloss as nearly life-like as possible. When the visitor stands on one side of the gallery, the animals thus appear as if living, while from the other side he observes the skeleton and realises its relation to the soft parts. The four principal specimens are a whalebone whale (*Balaena biscayensis*) from Iceland, 49 feet in length; a fin-whale (*Balaenoptera musculus*) from the Moray Firth, 69 feet long; a smaller fin-whale (*Balaenoptera borealis*) caught in the Thames near Tilbury; and a gigantic sperm-whale (*Physeter macrocephalus*) from Thurso, 54 feet in length. In addition to these there are other specimens, notably the mandible of a *Balaena* twice as large as the complete skeleton exhibited. We congratulate not only the Director of the Museum who has devised and superintended this important new departure in the exposition of zoology, but also Mr Edward Gerrard, junr., and his staff, who have so admirably carried out the technical part of the work.

Anthropology in Madras and in London

Mr Edgar Thurston contributes to *Nature* of May 26 a remarkably interesting account of the anthropological survey which he is carrying out in the Madras Presidency. European influence is bringing about a rapid change among the natives of Southern India, and there is no time to lose in taking note of their characteristics. As it is always interesting to see ourselves as others see us, we quote Mr Thurston's final paragraph. . . . "I gathered from observation when in London (1) that man as a social and intellectual being is illustrated with the unavoidable want of proportion, when no systematic scheme for the regular expansion of the collections is at work at the British Museum, Bloomsbury; (2) that it is under contemplation to illustrate man and the varieties of the human family from a purely animal point of view at the British Museum (Natural History), South Kensington; (3) that skulls must be sought for at the Royal College of Surgeons, Lincoln's Inn Fields; (4) that lectures and anthropological literature are available to members at the Anthropological Institute, Hanover Square. To this must be added (5) Mr Galton's laboratory. Surely a great want of centralisation, such as might well be remedied, is indicated here. And as I wandered, both in and out of the London season, through the deserted galleries of the Imperial Institute, I could not refrain from speculating whether, with a radical change of policy for good, this much-discussed building could not be converted into our great National Museum of Ethnology, where man shall be represented fully and in every aspect, and where those interested in ethnological research could find under one roof a skilled staff to appeal to in their amateur difficulties, collections, literature, lectures, and anthropological laboratory."

Recent Anthropology

To *L'Anthropologie* for January and February 1898, Dr I. H. F. Kohlbrugge contributes a paper upon the "Anthropology of the Tenggerois of Java," in which a detailed description of the physical characteristics of that people is given. They are referred to the 'Indonesian' race, with a slight admixture of Malayan blood. The average cranial index of 130 measured natives was found to be 79·71, mesaticephalic. There are several interesting tables in which comparative measurements are given for a number of races in the Malayan region.

Dr Salomon Reinach gives a detailed description of an interesting carving in steatite, representing a nude female figure, discovered in 1884 in one of the caverns at Mentone (Barma Grande) by Mr Julien. Two plates from photographs of this specimen are given, and show it to be of very rude workmanship. A gross exaggeration of the general form and an absence of detail

characterise this early attempt at representing the human form. The figure is interesting when brought into comparison with other early statuettes from Laugerie-Basse and Brassempouy. It is now in the Musée de Saint-Germain, near Paris.

A paper by Cecil Torr aims at showing that the so-called ship-designs upon certain ancient Egyptian pottery vases, are in reality representations of ramparts with towers, etc. This certainly seems a more plausible explanation than the ship-theory, but there is, unfortunately, no proof that even this interpretation is the right one. It is well enough *faute de mieux.*

Among the 'Miscellanea' there is to be noted an account of Anthropological work done in Spain and Portugal in 1897. There is evidence of considerable activity in this science in its various branches. In fact it appears to have been the most progressive of all the sciences during the year.

The Calaveras Skull

In 1886 Mr Mattison, who was prospecting in Calaveras County, California, sunk a shaft through four beds of lava down to the auriferous gravels at a depth of 127 feet. History does not relate how much gold he found, but all the world was soon aware that he discovered at the bottom of his shaft a human skull along with small human bones and other objects. In the same gravels, beneath the lava beds, there have also been found a rude stone pestle and mortar, and a dish of steatite. The skull is generally considered to be of an ancient type of structure, but many authors have considered the worked objects to be of somewhat advanced character. We have repeated this story because the skull and other objects, which belonged to the late Prof. J. D. Whitney, have recently been presented by his sister, Miss Maria Whitney, to the Peabody Museum of American Archaeology and Ethnology at Cambridge, Mass. At the time of its discovery the skull naturally caused great commotion in the scientific world, and the echoes of the discussion even reached literary men. At all events it is unnecessary for us to repeat the well-known Address of Bret Harte to the Pliocene Skull, in which the poet expressed his view by making the Skull reply:

> "Which my name is Bowers, and my crust was busted
> Falling down a shaft in Calaveras County:
> But I'd take it kindly if you'd send the pieces
> Home to old Missouri!"

The Geological Controversy in Austria

Austrian geologists have been for some time agitated by a dispute between Dr Alexander Bittner and Professor E. von Mojsisovics

regarding the nomenclature of the subdivisions of the Trias. Bittner accuses Mojsisovics of having renamed a stage that he himself had already named, by altering the meaning of his own name of Norische. Bittner holds that 'Norische' should be retained for the stage to which Mojsisovics originally applied it, and that Bittner's name 'Ladinische' should be accepted for the 'sub-norische' stage. The controversy has been carried on by Bittner with a vehemence which his English friends have regretted. He has, for example, written papers on Triassic nomenclature entitled 'Mojsisovics and Public Morals.' The question has now reached a more acute stage, and an appeal has been sent to European geologists by forty-eight Austro-Hungarian geologists, who state the case on behalf of Bittner, and appeal that his system should be adopted. This memorial has called forth several replies. Professor Rudolf Hoernes deplores that Austrian geologists should waste their time in such a dispute; and a letter to Mojsisovics signed by Professors E. Suess, Diener, Hoernes, Reyer, and Paul, refers to his brilliant zonal work on the Trias, and gives general support to his views on the particular question at issue. Professor Renevier points out that the term 'Noric' is pre-occupied in American geology, and therefore should be abandoned from Triassic geology. But fortunately the principle of priority has not yet been adopted in stratigraphy. Mr Renevier's compromise is open to the same objection that applies to Bittner's criticism on Mojsisovics. A mere appeal to priority is useless. The better system ought to survive.

The question at issue may be illustrated by the following table:—

		Mojsisovics.		Bittner.
		1st Scheme.	2nd Scheme.	
Upper Trias.	Upper	Karnische.	...	Karnische.
	Lower	Norische =	Juvavische.	Norische.
			Norische.	Ladinische.

Bittner's complaint is that Mojsisovics has changed the meaning of the term 'Norische' from the beds to which he first gave it, and applied it to those which Bittner had called 'Ladinische,' and that in order to do that he has proposed the new term of 'Juvavische.'

We express no opinion on the rights of the controversy, but we cannot help regretting that Herr Bittner's friends should have tried to settle the question suddenly by a referendum to the general body of geologists, whereas it is a question which experts on Triassic stratigraphy would gradually decide by the adoption of the most convenient and suitable classification. Like our Cambro-Silurian controversy time should be allowed to settle the question by the natural process of the survival of the fittest.

Westralian Water-supply

One of the difficulties in the Coolgardie and Kalgoorlie Goldfields is the absence of water. The success that has attended the sinking of Artesian wells in the sister colonies, notably in Queensland, to which we have often referred, suggested that similar action might be taken with profit in Western Australia. Mr A. Gibb Maitland, however, the Government Geologist, in a report that he has just sent us, comes to most pessimistic conclusions. The Coolgardie country consists for the most part of granitic rocks, which are weathered on the surface so as to form a superficial water-bearing layer of no great depth, but yielding enough water for ordinary purposes in certain spots; this water, however, is usually brackish. Below this weathered zone, however, none of the rocks are sufficiently porous to allow of the absorption and transmission of water; and since they are likely to be still more compact at greater depths there is small hope of obtaining a supply from that source. Very much the same conditions obtain at Kalgoorlie, and Mr Maitland does not recommend the continuance of any deep borings. There is great demand for water at Cue to work the crushing plants, but the nearest locality from which water can be obtained is Millie Soak, about ten miles to the north-east. Here is a bed of magnesian limestone, in which there have already been sunk wells that yield a supply of 1000 gallons *per diem*. The catchment area, however, does not appear to be large, and the quantity of water depends largely upon seasonal rains, so that the bed could not withstand a constant daily drain upon it of a quarter of a million gallons, which is the amount required. From Mr Maitland's refusal to recommend further deep borings we assume that the conditions which govern deep-water-supply in Western Australia are not the same as those that obtain in Sweden, where, as Baron Nordenskiöld has shown, fresh water can always be obtained at a depth of 30-40 metres below sea-level.

Corn-Midges and their Enemies

Of high scientific and practical interest is Dr P. Marchal's recent paper, "Les Cécidomyies des Céréales et leurs Parasites" (*Ann. Soc. Ent. France*, 1897, pp. 1-105, pls. 1-8). The famous Hessian Fly (*Cecidomyia destructor*) is naturally treated at greatest length, the three forms of its larva and the formation of the puparium being described and figured with many details. The parasites which are of the greatest service in keeping the midges in check, are mostly minute hymenopterous grubs (Chalcids and Proctotrupids). Among the latter, *Trichacis remulus*, Walker (parasitic on *Cecidomyia Avenae*, Marchal) is described in detail. Its first larva is cyclops-like. Three or four of these live on the nervous system of a cecidomyid larva, and the nerves of the host degenerate with the

formation of 'giant cells.' Later the *Trichacis* larva becomes maggot-like. The grubs of another proctrotrupid *Polygnotus minutus* live, in their early stage, grouped in the food-canal of the midge-larva. They ultimately devour the whole body of their host except the outer skin.

Economic Entomology

We have received several recent *Bulletins* of the Entomological Division of the U.S. Department of Agriculture. No. 10 contains a series of short miscellaneous papers. Of special interest are Dr Zehnter's on caterpillars which bore sugar-canes in Java, and Mr Matsumura's on two fruit-boring caterpillars of Japan, one of which injures apples and the other pears, after the fashion of the caterpillar of our own codlin moth. No. 12 contains the story by Mr L. O. Howard, of the never-failing San José scale (*Aspidiotus perniciosus*) during 1896 and 1897, from which it appears that a united effort on the part of fruit-growers to cope with this pest is being made. In February of the present year, the German Government issued an order to stop the importation of American produce infected with the insect. No. 13 is a compilation of the recent laws, both state and federal, against Injurious Insects in North America.

The San José scale also occupies much space in the Twentieth Report of the State Entomologist of Illinois (Mr S. A. Forbes). A curious observation is given in this Report on the habits of a species of solitary wasp (*Odynerus foraminatus*) which by making its mud-nest in the air-opening of a railway automatic brake, has, on several occasions, rendered the release of the brakes impossible and caused delay and danger to the trains.

British farmers and tree-growers will welcome as usual Miss Ormerod's Twenty-first Report on Injurious Insects, recently issued. Together with notes of value on more familiar insects, we notice (pp. 34-40) some specially interesting observations on the development of vestigial wings in the female of the Deer Forest Fly—*Lipoptena cervi*—one of the pupiparous Diptera, and the latest experiments in lessening the damage done to fruit-bushes by the Currant Gall Mite (*Phytoptus ribis*).

North American Leaf-Hoppers

Mr C. P. Gillette's revision of the American Typhlocybinae (*Proc. U.S. Nat. Mus.*, vol. xx., pp. 709-773) contains—as might be expected in so neglected a group—a large proportion of new species, most of which are illustrated by clear structural figures. As several forms are common to both sides of the Atlantic, Mr Gillette's paper will be useful to European workers. He unites several genera which have hitherto been held distinct, not finding the differentiating characters constant.

North American Land Shells

"A classified catalogue with localities of the Land Shells of America north of Mexico," compiled by H. A. Pilsbry and C. W. Johnson, is a small brochure of thirty-five pages, reprinted from *The Nautilus*. The great advance in our knowledge of the true relationships of the members of the great Helicoid group as brought about by Pilsbry's work has rendered the production of such a catalogue as this most desirable, and one which will be greatly appreciated by all who are interested in North American Land Mollusca; whilst it further shows a considerable increase in the known number of species from that region since the last edition of Binney's "Manual," which appeared in 1885.

Whether all these extra species, some seventy-five in number, will ultimately prove valid, time alone can show; but we confess to feeling very sceptical about some. Certain species are acknowledged imports.

The grouping of the larger families does not strike one as altogether happy or even natural. The insertion of the Agnatha between the Holopoda and the Aulacopoda is especially unfortunate. We may point out that *Vitrea draparnaldi* is a synonym of *V. lucida* (Drap.).

The Connop Collection of British Birds

East Anglia has long been famous for the richness of its 'Ornis,' and the researches of the ornithologists who dwell within its borders have contributed in no small degree to our knowledge of the birds of Western Europe. Foremost among Norfolk naturalists of the present day stand Mr J. H. Gurney and Mr T. Southwell; the latter having completed the third volume of poor Stevenson's "Birds of Norfolk" in the most praiseworthy style. It is to the zeal of these two gentlemen, and more especially to that of Mr Southwell, that we are indebted for a precise history of the ornithological treasures to be studied at Rollesby Hall in the form of a catalogue some sixty pages in length. Mr E. M. Connop of that place is keenly interested in local zoology. During the last thirty years he has used every opportunity of securing for his collection the rarest birds procured by the Norfolk wildfowlers. How far his efforts have been rewarded with success may be guessed from the fact that he is the proud possessor of four local specimens of the White-eyed Pochard (*Nyroca ferruginea*), two local examples of the Gull-billed Tern (*Sterna anglica*); and three individuals of Sabine's Gull (*Xema sabinii*) obtained on the Norfolk coast. It is satisfactory to know the exact whereabouts of such specimens as the only examples of Pallas' Warbler (*Phylloscopus proregulus*) and the Mediterranean Herring Gull (*Larus cachinnans*), that have been so far detected within the limits of the British Isles. Strange to say, the Whooper Swan (*Cygnus musicus*) has not been included in this fine series.

I

A New Reading for the Annulate Ancestry of the Vertebrata

THE question of the ancestry of the vertebrates being still unanswered, anyone is at liberty to make suggestions. No new facts seem to be forthcoming to enlighten us; we are driven therefore to find new readings of the old. To those who scorn all theorizing, and are content to wait until the new facts turn up, I would suggest the following questions: Are we sure we have read all that the old facts have to teach us? Have we arranged them in every possible order, and are we competent to deny that they can yield us any clue to the solution of the problem?

I ask these questions somewhat feelingly, because I have recently lighted upon a new way of arranging the old facts, and I propose to offer it to my fellow-zoologists for what it is worth. This much, indeed, I claim for it, viz., that it shows a way of escaping from at least some of the difficulties in the way of the annulate origin of vertebrates. It provides us with another escape from having to assume that the annulate ancestor, with its ventral nerve-cord, turned over on to its back to become the vertebrate with its dorsal nerve-cord; and it shows how the notochord and neural plate, those most characteristic of all vertebrate structures, might have been unsegmented from the beginning as secondary developments within an originally segmented body. I propose, in short, to show how the assumption of a primitive hirudinean as our ancestral annulate enables us so to re-arrange the old facts as to bridge over the gulf between the invertebrates and vertebrates with startling ease. I do not affirm that our nearest invertebrate ancestor was a hirudinean. I only wish to show how it is conceivable that a primitive leech might have developed into a low vertebrate form allied to the cyclostomes.

My attention was first directed to the hirudineans by the fact that the embryonic muscles of cyclostomes and sharks are of the same type as are the muscles of the leech. I am well aware that histological resemblances are in themselves of no value to morphology. In this case the resemblance served to suggest the hirudineans as the possible annulate ancestors of the cyclostomes. As is well known, they, like the cyclostomes, have no appendages, no

setae, a rich secretion of slime by the epidermal cells, and a somewhat similar method of feeding. It was this last fact which definitely rivetted my attention. It is true that it appears trivial enough at first sight, but the longer it is considered, the more weight does it seem to me to possess.

Described in general terms, both the leech and the cyclostomes attack living prey by their mouths, and bite into it with buccal teeth. I cannot recall a single other instance of this method of feeding, the nearest approach to it being that of certain molluscs which seize their prey with the radula. Elsewhere, we have limbs modified into jaws in abundance, or pincers, or protrusible proboscides shot out and drawn back with the prey adhering; or, again, tentacles capture food, or simple cilia set up currents of water and sweep particles of food into the mouth; but only in the leeches and the lower vertebrates do we have, so far as I know, the seizing of living prey in the manner described.

It may perhaps be remembered that I have already, on several occasions, expressed my conviction that the profoundest morphological transformations leading to the rise of new groups of animals can be traced to the adoption of new methods of feeding. This appears to me such a self-evident proposition that it ought to be almost unnecessary to repeat it,[1] yet I am not aware that it has ever been applied systematically except in the two cases in which I have myself endeavoured to apply it. My maiden zoological treatise[2] (apart from a small preliminary note) was an endeavour to show that the primitive crustacean, whose nearest existing relative is *Apus*, could be deduced from a chaetopod annelid by its adoption of a browsing manner of life in such a way that it could use its parapodia for pushing food towards and into its mouth. I endeavoured to show that the new and richer food-supply which this co-operation of limbs and mouth yielded gave rise to a new race, which could be shown to include not only all the existing Crustacea with their marvellous wealth of varied forms adapted to almost every conceivable environment, but also the trilobites and the Gigantostraca. The somewhat hostile reception accorded to this little work (which, no doubt, for many subsidiary reasons, it deserved), did not shake my conviction that the principle adopted was sound, and that, in order to understand the essential morphology

[1] One objection suggested to me is, that the proposition assumes the inheritance of acquired characters. On this point I have already stated my views (*Nature*, vol. I., p. 546. 1894), but in the meantime it is worth asking whether it really does assume anything of the kind. Say that new pastures call for a new method of feeding, which, again, requires certain structural adaptations, can the rise of the latter not be explained by a process of natural selection, the new requirements for success in life weeding out all whose congenital variations were not in the required direction? For my own part, I can see no difficulty in believing in inheritance. The objection as to the absence of proof begs the question as to what is meant by proof.

[2] The Apodidae. Nature Series. London: Macmillan & Co. 1892.

of any group of animals, we must, if possible, discover the method of feeding which caused the ancestral form to depart from its congeners. Strong in this conviction, therefore, I made a special comparative study of the Arachnida, extending over four years, and ultimately endeavoured to show[1] that their peculiar morphology could be explained in detail as an adaptation to their method of feeding.

It was therefore only natural that any suggestion, even the faintest, as to what might have been the primitive method of feeding of the ancestors of the vertebrates should lead me at once to see whether the same principle could not be made to apply again. Was it not possible to deduce the typical low vertebrate from a hirudinean by a series of structural modifications resulting from a further development of the leech-method of feeding? No one will deny that such a possible solution was worthy of investigation, even though he may not be so convinced as I am that morphology is an aimless pursuit unless it go hand in hand with physiology. Although most zoologists admit this latter as a pious opinion, in practice it is too often ignored, as may be gathered from the fact that every attempt to discover the ancestry of the Vertebrata, with which I am acquainted, has been based solely on structural similarities, while the functions of the structures themselves have been treated in a most arbitrary fashion. For example: the central nervous system of the Arachnida is said to have become the central nervous system of the Vertebrata, with an entirely different organism to be innervated. The intestine of the king-crab is said to have been lost in the spinal cord of the Vertebrata, and a new one has been provided; the sheath of a protrusible proboscis is turned into the vertebrate notochord; old mouths may close and new ones open, and so on. Continuity of function is apparently of very secondary importance, while similarity of structure or of mere position relative to other organs is of prime importance. I do not call this physiology and morphology going hand in hand. It seems to me more like physiology being dragged by the neck, while the morphologist demonstrates the perfection of his structural resemblances. Hence, all the arguments to which we have hitherto been accustomed have seemed to me from the first to be hopeless. It is not alone to the discovery of similarities of structure that we must look for clues to evolutionary progress, but rather to the development of new functions in response to some probably gradual change in the environment, these new functions leading, whether by selection or inheritance, or both together, to modifications of structure, subject always to the physical laws of the environment.

[1] "Comparative Morph. of the Galeodidae."—*Trans. Linnean Soc.*, vi., pp. 305-417, 1896.

In what follows, therefore, I propose to apply the last-named method to the Hirudinea, and to enquire whether the structural changes which might be expected to follow from a further development of their method of feeding would not transform them into Vertebrata. I am quite aware that the argument itself may from first to last be merely an academical discussion without direct value to Morphological science. But it has been suggested to me that merely as an essay in Physiological adaptation, especially as it is here applied to a 'burning' question, it may do good, by calling attention to this method itself, and perhaps lead to useful discussion as to the soundness of the principle and the extent of its applicability.

Let us then assume a race of hirudineans not yet so specialised as our medicinal leech with its dorso-ventrally flattened body and terminal sucker, probably a further specialisation of this flattening. We will assume that they lived freely in the open sea, chasing their prey with true serpentine motion, and attacking it with open mouths, armed with buccal teeth. Let us assume that this method of obtaining food was eminently successful, no very arbitrary assumption, because we may, as a rule, assume that a new method of feeding is adopted, because a rich and hitherto untapped food-supply offers itself. Let us, then, consider how such animals as we have pictured, viz., free-swimming rapacious leeches, might possibly develop, given an abundant food-supply.

The first results would be growth in size, larger mouths and more teeth.[1] These would lead to a very important change in the character of the diet—viz., to the swallowing of a great deal of solid food. Small animals would be gulped down whole, while lumps would be torn out of larger ones too big to swallow. This change would profoundly influence the alimentary system. Solid food demands a slow passage down the alimentary canal while it is being digested, and a full meal of such solid foods would entail a great distension of the anterior section of the alimentary canal, the ultimately differentiated stomach.

Now it seems to me that a swollen anterior portion of the alimentary canal, heavy with solid food—and we have every reason to believe that the full meal was a frequent occurrence—would necessitate, besides the formation of a muscular stomach, other and more profound changes in the organisation. In the paper above referred to on the morphology of the Arachnida, I have endeavoured to show that almost every detail of the organisation of that group of animals has been profoundly modified by the necessity for adaptation to the full-meal condition. Unlike our hypothetical hirudinean ancestors, the Arachnida have never given up pure blood-sucking, and

[1] The change from chitin to horn might be correlated with the change from the unilaminate to the multilaminate epidermis.

to ensure the purity of the food have developed a beautiful variety of sieves and strainers to prevent any solid matter from passing into the alimentary canal. With this purely liquid food, they manage to distend themselves almost to bursting, by a kind of force-pump action of the oesophagus. The effect of this distension of the alimentary canal upon itself and upon the other organs of the body can be made to explain all the more important structural peculiarities and variations in the Arachnida. No single organ or assemblage of organs has remained unaffected; all have had to adapt themselves or protect themselves. The whole form of the body has been changed, limbs have aborted, and the respiratory and circulatory systems have been profoundly modified. So obvious is this to any one who studies the group from this point of view, that we are quite justified in postulating modifications and adaptations of the organisation of our assumed hirudinean ancestor, not only to the full-meal condition of its alimentary canal, but also to its more constant load of solid lumps.

In the Arachnida, one necessary precaution was the protection of the muscular and nervous apparatus against temporary incapacity due to the distension of the alimentary system. This has been brought about by a division of labour, one region of the body undertaking the animal (locomotory and sensory) functions, the other the vegetative and digestive functions. The arachnidan body accordingly is divided transversely by a narrow waist or diaphragm; the alimentary canal in the posterior division can be distended to its utmost limit without pressing at all on the anterior part. The question as to which region should be the animal and which the vegetative was naturally settled by the fact that the muscular apparatus of the jaws and of the capturing limbs, and the ganglia of the great sensory organs were already at the anterior end of the body.

Now I assume—and in this assumption lies my new reading of the facts—that our supposed hirudinean had to undergo modifications for precisely the same end, viz., the protection of the locomotor functions from the alimentary. I again suggest that a division of labour took place, the body dividing not transversely, as in the Arachnida, but longitudinally, *i.e.*, into a dorsal and a ventral half; and then, that the dorsal half had to protect itself from the ventral.

We will deal first of all with this assumed division of labour. The weight of the distended abdomen pressing downwards upon the primitive ventral nerve-cord and muscles would seriously affect their working; while, as some compensation for this loss of power, the dorsal neuro-muscular system, on which at any rate the pressure due to gravitation did not act, would be free to fulfil its functions, and thus able to develop in order to meet the greater strains

put upon it, *i.e.*, if the assumed active life was to be maintained. Such a division of the body is the only one which affords not only direct continuity, but also the closest possible association, between the dorsal neuro-muscular region and the great ganglia of the sensory organs at the anterior end of the body. That such a division of labour actually existed between the dorsal and ventral halves of the primitive vertebrate body, the former being the muscular and locomotor region while the latter was the vegetative region, is shown by the transverse section through the middle of the body of a low vertebrate.

This, then, is our fundamental hypothesis: that, as our annulate ancestors, rapacious hirudineans, grew in size, and developed larger mouths and throats, a change of diet took place, in that small animals and lumps of solid food were swallowed; further, that the new burden thus thrown on to the system led to a division of labour between the dorsal and ventral halves, the former tending to monopolise the neuro-muscular functions, the latter the vegetative. When, however, I refer to the transverse section through the trunk of a vertebrate, and point to the fact that such a division of labour actually existed, I must not be thought to assert that this was brought about in the manner described, only that it is conceivable that it might have been so brought about. I assume that it has been so brought about merely for the sake of showing that if this is granted it would lead to further structural changes capable of transforming our hirudinean into a vertebrate.

In all that follows, then, we have to keep before our minds our soft-bodied vermiform ancestor with a longer or shorter anterior section of his alimentary canal distended by lumps of solid food; the weight of this food pressing downwards, the dorsal muscles would be slightly easier than the ventral, which would be seriously incapacitated; hence a possible cause for the gradual differentiation of the two regions, the dorsal, as already stated, tending to take over the animal, the ventral the vegetative functions.

Now it seems to me that the more highly differentiated the body became in the direction suggested, the more necessary would it be to protect the one division from the other. The primary division of labour was supposed to be due to the constant weighting and periodical distension of the alimentary canal with solid food. The more capacious the alimentary canal became — assuming its increased development as it gradually acquired a region of its own — the greater the possibility of distension. Thus the danger to the dorsal region from being incapacitated by pressure from the ventral region is not removed; it is rather increased, unless the two regions are mutually protected from one another.

In the Arachnida, in which the distension is sometimes positively

monstrous, the end is gained by a waist (or, as in *Scorpio*, a perforated diaphragm, which is only a waist with the infolded external faces fused together). From the arrangement of the muscles, it appears that the narrow neck of this waist can be constricted when necessary. No such method of protection, of course, is conceivable in the case of the primitive vertebrates, in which a great part of the dorsal region of the body had to be protected from the ventral half of the same region. That protection was as necessary as it is in the case of the arachnids, we may surely believe if the rotundity of the body of the tadpole is any sort of repetition of the state of periodic distension of our early ancestors, although of course in the tadpoles later modifications, such as the coiling of the intestine and the forward movement of the vent, are already superposed.

The method of protection which actually was adopted—reading now from the embryological record—seems to have been as follows. A dorsal strip of the alimentary canal thickened and eventually separated off as the functions of the canal demanded free play to cope with the increasingly difficult digestive problems which the developing mouth and teeth, in their quest of new things to devour, continually sent down for solution. This thickening of a dorsal strip of the alimentary canal may perhaps again be referred to the downward pressure of the food at all times, whether the alimentary canal was distended or not. The dorsal epithelium would only be seriously pressed upon in the condition of actual distension, the ventral would be subjected to pressure whenever there was any solid food to rest upon it.

It is conceivable that this dorsal strip of endoderm might have remained a protective plate if its sole function had been to screen the neuro-muscular system from the distension and churning activity of the alimentary canal. But, at the same time, it served another and almost equally important purpose which led to its stiffening longitudinally into an elastic rod. It is, indeed, horrible to contemplate the possible fate of our unfortunate ancestors if, while the ventral half were fully distended with food, some sudden stimulus compelled the dorsal half violently to contract, as we know the modern leech can contract, to less than a quarter of its length. A stiff rod along the back alone could avert such a catastrophe. Hence I would suggest that the protective strip of endoderm stiffened longitudinally as it was progressively differentiated from the alimentary canal, and further, narrowed as it thickened, so as to permit of free serpentine movements of the body. We may then leave it as an elastic rod protecting on the one hand the spinal cord —which, as we shall see, must have been concurrently developed— from functional disturbance by the alimentary canal, and on the other, the alimentary canal from possible mechanical injury due to

sudden contractions of the dorsal muscles. Its subsequent development, as it became invested with cartilaginous and bony rings, is already within the vertebrate domain.

The spinal cord.—Great muscular development is impossible without corresponding nerve-development. Hence it seems to me, if the division of labour here assumed is admitted, a nerve-strand would develop down the middle between the muscular bands, not necessarily as an altogether new structure but as a new condensation of elements probably already present. I have always hitherto been of the opinion that the embryological nerve-plate, which subsequently forms the well-known groove and neurenteric canal, was the remains of larval adaptations ; but from the point of view now suggested it appears that the process might actually represent, in a very abbreviated form, the gathering together of the originally scattered nerves which supplied the dorsal muscular region into a central strand for more perfect co-ordination. Perhaps, also, since respiration in the Hirudinea is effected solely by the skin, the neurenteric canal may have been a temporary arrangement for aerating this important nerve area as Sedgwick[1] and Van Wijhe[2] suggested long ago. Be this as it may, the appearance of the spinal cord itself, possibly as a new development of pre-existing elements, could be considered as a natural consequence of the division of labour above postulated. With regard to the great development of the anterior portion of the spinal cord, the brain, we should not be far wrong if we referred this to a great improvement in the organs of sense required by the new race of swift, rapacious carnivores.

Concurrently with the development of this new nerve-cord, the primitive ventral nerve-cord of the annulate would be slowly degenerating, not only because of the transference of the chief muscular activity to the dorsal region, but because the ganglionic chain itself would be positively incapacitated from fulfilling its functions not only by the periodical distension but by the more constant pressure of the alimentary canal weighted with solid food. In this comparatively simple manner, then, I suggest that the problem of the nerve-cords might be solved, and one of the difficulties in the way of the annulate ancestry of the vertebrates be avoided. Of course, it must remain a matter of opinion whether the assumptions here made are really preferable to the (to my mind) desperate hypothesis otherwise difficult to avoid, that our worm ancestor turned over on to its back, that its ventral cord became our dorsal cord, and that its old mouth vanished and a new one developed.

Concurrently with these specialisations of the dorsal neuro-

[1] *Proc. Cambridge Phil. Soc.*, iv., p. 325 ; 1883.
[2] *Zool. Anz.*, 1884, p. 683.

muscular region, modifications would have been taking place in the ventral vegetative region, always in adaptation to the new functions of the alimentary canal. This canal, besides being greatly distended periodically, would almost always be weighted with lumps of solid food.

Within this ventral region we may suppose that, in addition to the alimentary canal, we should have all the main trunks of the circulatory system, the excretory organs (segmental nephridia) and the genital bodies. Let us see how the new burdens which we imagine to have been thrown on the alimentary canal might be expected to affect not only these, but indirectly also the respiration, which is always closely associated with the circulation.

The Circulatory System.—In the paper on the Arachnida above quoted I have already given an outline sketch of the profound changes which the method of feeding of the Arachnida has necessitated in the circulation of the different arachnidan families. No less striking should be the changes produced in the circulation of the primitive vertebrate when a fully distended stomach pressed against the developing notochord dorsally, while ventrally and laterally it stretched the skin to its fullest extent. We are justified in assuming that the principal blood-vessels of our hypothetical hirudinean ancestor ran longitudinally. I suggest that the distended alimentary canal would seriously hinder the passage of blood along these vessels, and we might expect a congestion both in front of and behind the obstructing swelling of the alimentary canal. The anterior congestion of the vessels is that which alone concerns us. It would lead to their distension both transversely and longitudinally, the latter distension being accompanied by some degree of coiling. It is further conceivable that at some portion of the congested system a thickened muscular tunic would be developed to cope with the difficulty, the thickening tunic perhaps involving the coils, so that a specialised heart might be developed, capable of forcibly pumping the blood past any such obstruction as the alimentary canal could cause. That the anterior and not the posterior congestion would give rise to the heart we may infer from its proximity to the central nervous system, which would, doubtless, be in some way connected with the muscles causing the pulsation of the primitive vessels.

Respiration.—The intimate connection which exists between the circulation and the respiration is so well established that it needs no emphasis here. I call attention to it merely to show that, if the mechanism of the circulation became localised, as suggested, at the anterior end of the body, so the respiration would tend to be localised near the heart. Here again the diffused cutaneous respiration of the Hirudinea, with their capillaries even entering the epidermis,

might supply us with the possible conditions out of which the breathing organs of the early vertebrates could have developed. While the stretching of the ventral and lateral skin by the distended stomach would hinder circulation in the areas thus affected, the pulsing of the increasingly powerful heart would distend the cutaneous vessels nearest to it, *i.e.*, in the neck region. Hence a possible origin of the external gills. At the same time, the rapid swimming through the water with the mouth frequently, if not always, open for the capture of prey, would keep an almost constant supply of fresh water in the pharynx. Here, also, we should, on account of its proximity to the heart, expect a subsidiary respiratory surface to develop. The actual processes which would lead to the union of the inner and outer respiratory surfaces by means of gill-clefts, it is not easy now even to conjecture. The best suggestion which occurs to me is, that they may have been due to the well-known principle of the increase of respiratory surface by plication, which would inevitably bring the portions of the inner and outer surfaces into increasingly close proximity. We can, however, readily estimate some of the advantages of this development. The clefts would insure a fresh stream of water through the pharynx, this stream would aerate the posterior of the external gills, which, in rapid swimming would be folded back against the body, and thus be screened from the necessary contact with the medium by the anterior gills. Lastly, the clefts afforded retreats into which all the external gills ultimately withdrew, their presence on the exterior being a hindrance to locomotion, and a source of danger in the event of attack.

Excretion.—In the more primitive of the hirudineans, the segmental organs (nephridia) are arranged laterally along each side of the under surface of the body. It would of course be imperative to protect these from all injurious pressure from the distended alimentary canal. In order to escape this, their relative position might have been changed, and changed in the following way. The downward distension of the alimentary canal might be expected to force the two rows of primitive nephridia apart until, instead of lying laterally below the alimentary canal, they would come to lie laterally above that organ. This, we might suppose, would be the first change of position, brought about perhaps mechanically and also perhaps partly physiologically, inasmuch as the downwardly pressing intestine would permit a slightly freer circulation above it than below it. A further change would take place when the organs became concentrated behind the stomach; again, no doubt, in order still further to escape from pressure from that organ.

The reproductive bodies.—There is no difficulty in under-

standing how the same factors which concentrated the kidneys behind the stomach would also assign the same place to the genital bodies. In the Arachnida the genital bodies have to accommodate themselves to the spaces left among the caeca of the alimentary system.

The body-cavity.—There has hitherto been no satisfactory reconciliation of the embryological facts that, while the neural plate and notochord, the two most characteristic vertebrate structures, are primitively unsegmented, apparently indicating an unsegmented ancestral form, the body-cavity appears as a definite series of coelomic cavities (*e.g.* in *Amphioxus*) apparently indicating equally emphatically that the ancestral form was segmented. A confusion of types, if ever there was one! Now I make bold to suggest that the assumed modifications here sketched supply us with a possible solution. The series of archenteric cavities which, in our annulate ancestors, encircled the body below the skin, were gradually prevented, in the early vertebrates, from invading the dorsal region, because that region in becoming secondarily specialised into a neuro-muscular region, essential to the free life of the larva, was developed as early as possible in the ontogeny. Hence, in the embryo, the metamerism is confined to the vegetative region; its invasion of the dorsal region is secondary, in that a gradual impress is made upon the originally unsegmented notochord and on the points of departure of the spinal nerves by the segmented muscles and developing skeletal rings.

The moving forward of the anus and the coiling of the alimentary canal.—The periodic distension of the stomach, by stretching the walls of the body, would tend to form a space into which the rest of the alimentary canal would gradually withdraw, simply from following the direction of least pressure. This process might be furthered by the additional advantage to the animal of a tail which could be used as a purely locomotory organ unhampered by any other organs. For it is obvious, on the one hand, that a rectum periodically charged with more or less solid faeces, the residue of the solid matter swallowed, would seriously impede the free movements of such a tail, while, on the other hand, the distension of the middle body, hindering the original serpentine motion by which our annulate ancestors progressed through the water, would lead to the necessity of gradually specialising the tail as the chief organ of locomotion. The forward movement of the anus and the coiling of the intestine would therefore be natural results of this new differentiation of the body into a swollen anterior portion and a flexible tail (*cf.* the developing tadpole).

Before concluding, I wish again to emphasise the drift of this article, which is simply to suggest a possible method by which the

gulf which separates the invertebrates from the lowest vertebrates, the cyclostomes, can be bridged. I do not see how it is possible to demonstrate either the strength or the weakness of the particular bridge here sketched. It is enough if it is merely kept in mind as a hypothesis, that is by those to whom it appeals. Beyond suggesting this possible method of bringing about the more fundamental morphological changes which transformed the invertebrate into the vertebrate, the hypothesis does not profess to go; the subsequent development of skeleton, limbs, jaws, teeth, &c., belongs to vertebrate morphology as such.

One possible test, viz., that of direct transition forms, is entirely lacking. If anything, however, this favours our argument. For if there be any truth in our suggestion we could hardly expect to find transition forms, at least certainly not fossilised, for such hirudinean ancestors would naturally be soft-bodied, and, by the time the notochord was developed in the manner sketched, they would be no longer hirudineans but primitive cyclostomes.

One class of vertebrate, however, we do find, which is apparently lower than the cyclostomes—*Amphioxus* and the Tunicata. Though these have long been recognised as related to the vertebrates, their position in the chain of evolution has been matter of endless controversy, for the obvious reason that the order of the chain is not known. It seems to me that the line of development from the invertebrate hirudinean to the vertebrate cyclostome which is here suggested would assign them a natural position. They are not true links in the ascending chain, but forms which have branched off and become differentiated, again, be it specially noted, in adaptation to a new method of feeding. We postulated for our earliest ancestors when they were just turning into vertebrates, an active free-swimming life, catching and devouring food with gaping jaws, and consequently with a stream of water flowing through the pharynx and out at the gill-slits, kept up by rapid swimming through the water. This constant stream of water through the pharynx would carry along with it innumerable small particles of food, which might with advantage be caught and turned down the oesophagus. It seems probable that all the early vertebrates developed an organ for this purpose, the endostyle, the particles perhaps attracted by and adhering to a slimy secretion which was then worked by ciliary motion into the oesophagus. Now, either in special regions where these small particles were unusually plentiful, while larger prey requiring great exertions and the use of teeth was scanty, or among very young forms not yet strong enough to attack prey with mouth and teeth,[1] this passive method of feeding was nearly sure to be adopted by a certain number of individuals as

[1] The larval amphibian has acquired a browsing herbivorous diet.

their normal method, and by becoming highly specialised may have carried those who were otherwise on the upward path of vigour and rapacity back into a quieter life. We may quite agree with Brooks that the free-swimming tunicates show no sign of degeneration, that is, judged by themselves, and we may admit their perfect adaptation to their method of life; but, none the less, according to our argument, the line of their development leads to the sedentary tunicates. Further, if there is any truth whatever in our sketch of the rise of the notochord, we must believe that the ancestors of *Appendicularia* once chased and seized prey with their buccal teeth, and swallowed more or less solid food, leading to periodical distension of the alimentary canal. All this has been lost; the seizing apparatus has vanished, and the alimentary canal requires to put forth no further serious digestive efforts, as the food-particles which dribble to it are easily disposed of.

We should then perhaps have to regard *Amphioxus* and *Appendicularia* as two distinct and separate offshoots from the advancing army of the new race, in both cases enticed, as it were, on one side to abandon the rapacious method of feeding for the easier and more passive method above described.

I have already suggested that no fossil transition forms between hirudineans and cyclostomes could be expected, and as a matter of fact, on turning to palaeontology, we find that when the palaeozoic vertebrates first appear they appear suddenly, as primitive fish without jaws or paired limbs. Only one form, *Palaeospondylus* of Traquair, from the Old Red Sandstone, is claimed to have been a cyclostome or lamprey. That only one such form is found, while the rest are shark-like in shape with heavy armour, does not seriously affect our argument. For we have no difficulty in assuming that the lamprey with its unarmoured skin is more primitive than these shark-like forms with their heavy defensive armour. Further, I should conclude that these armoured sharks implied the presence of unarmoured forms, which for that reason have left no remains. I should regard the armoured forms as having arisen as offshoots from the active advancing race—offshoots which have retired to a more peaceful life behind their secondarily acquired defences. I feel obliged to assume that these armoured sharks were so far degenerate forms and were not the ancestors of the later unarmoured true sharks, because I think that heavily-armoured creatures have practically ceased to advance. According to the principles here adopted, that the acquirements of new functions lead to structural adaptations, we see that passivity, or even activity if limited to narrow grooves, must be fatal to evolutionary progress. The Gigantostraca which have died out, with the exception of the stationary *Limulus*, are *(pace* Gaskell) examples of this. Further,

when we picture to ourselves the nature of the altered character in the struggle for existence which the new race introduced with their swift movements, their gaping jaws and pointed teeth, it is not astonishing that some forms should develop armour, even at the risk of degeneration. On these points, however, we may hope for further light from palaeontological research.

This, then, brings my argument to a close. I am aware that many of the points might have been more elaborately treated, but enough, I think, is contained in this sketch to serve all present purposes. The argument is quite clear, and so far as I can see consequential. What it is worth is another matter. I, for one, suspend my judgment.

HENRY M. BERNARD.

STREATHAM, S.W.

II

Botanical Work Wanting Workers [1]

THE field for investigation in botanical work is so extensive and increases so rapidly every year, as new discoveries open up new vistas of possible knowledge, that it will, doubtless, have been taken for granted that the title of this paper can refer only to those branches of the subject in which this Association is especially interested, viz., the flora of the four South Eastern Counties. Of these four counties I have limited myself to the County of Kent, as being the one with which I am most familiar, and as best serving my purpose, although the following remarks will probably apply with equal force to the other counties.

An examination of the various county floras that have already been published shows that the flowering plants have, as a rule, received much more attention than the cryptogams, and that in very few instances have adequate county lists of Mosses, Lichens, Fungi, or Algae been published. Such as have appeared are usually imperfect in one or other of the groups, especially as regards dates and localities, the Mosses and Fungi being generally those best represented. It is therefore this branch of botanical work which specially needs workers, and to which I wish to direct particular attention. A Flora of Kent is, I understand, now going to press, but includes at present only the flowering plants.

About twenty years ago an attempt was made to collate the various cryptogamic lists existing in small local floras, or in published reports of Kentish Natural History Societies, and to render them as complete as possible by further personal investigations. The results obtained were published by myself, so far as the mosses, scale-mosses, and lichens were concerned, in the *Journal of Botany* for 1877 and 1878, and my friend Mr T. Howse kindly undertook to add to and complete the manuscript list of Fungi, which I had in preparation. He subsequently published it in the same journal in 1879. But the marine and fresh-water Algae still remain to be worked out. The object in publishing these lists, in their admittedly incomplete state, was to induce isolated workers in different parts of the county to contribute such information as they might have accumulated. But except by those who were already working at

[1] A paper read before the S.E. Union of Scientific Societies, meeting at Croydon, on June 4, 1898.

the subject, and who since 1879 have found many of the species which were expected to occur, very few additional observations have been published. But it is hoped that the impetus given of late years to microscopic work will result in more naturalists directing their attention to this undeveloped side of county floras.

It may perhaps serve a useful purpose to direct attention, under the different groups of Cryptogams, to the work that still remains to be done in Kent, and to mention under each, the districts that have not yet been searched, so far as I am aware, and which are therefore most likely to repay investigation.

1. Mosses.—Since 1879 twenty-seven species have been found new to the county, but there still remain a number which have been suggested as likely to occur in Kent, since most of them have been found in the neighbouring counties of Surrey, Sussex, or Hampshire. There are, however, some parts of the Weald of Kent, especially those on the Sussex border, which have not been fully explored, but which may yield some of the moorland species. This is evident from the recent researches of Sir James Stirling, who has added several species to the county flora from the neighbourhood of Goudhurst. The mosses yet unrecorded are those likely to occur on hilly woods and moorlands, rocky shores, damp sand-rocks, and damp sand-hills near the sea.

The shores of Kent, between Dover and Deal, and the sand dunes on the east coast, as well as the marshes on the north coast of the county, may be expected to yield several species. This part of Kent, as well as the parks and woods about Canterbury, and between Ashford and Appledore, and in the neighbourhood of Cranbrook and Hawkhurst, and between Maidstone and Chatham and Sittingbourne, and between Wye and Folkestone, have not, I believe, been thoroughly explored, and would probably furnish other species.

The best time to search for mosses is from September to May, choosing as a rule damp days after showers, when the foliage is expanded. (A list of the species likely to occur in Kent is appended to this paper.)

2. Scalemosses.—The species unrecorded for the county are chiefly those which grow on damp sand-rocks and in boggy woods, and these also must be looked for near the Sussex boundary, or in the neighbourhood of Ashford. The best time of the year to search for them is from February to April. (A list of those that may possibly be found in Kent is given below.)

3. Fungi.—The number of species of Fungi occurring in Kent is comparatively large, but several parts of the county, particularly the districts alluded to under mosses, still need exploration. The groups which are still very badly represented are the Myxomycetes and Gasteromycetes, and some groups of the Coniomycetes, Hypho-

mycetes, and Ascomycetes, especially the Tuberacei. (A list of these groups is appended.)

The best time of year to search for Fungi is undoubtedly from July to November, although some may be found during the rest of the year.

The Tuberacei are probably neglected because very little indication of their presence underground is to be observed except by the eye of an expert. On digging up the mould under oak and fir trees their presence may sometimes be detected when the dead leaves are scraped away, by the appearance of a mycelium on the top of the soil, but more easily perhaps by the fact that the mould when dug up exhibits here and there firmer masses, due to its being compacted together by the nearly invisible mycelium, and these lumps when broken open show the more or less globular fungi inside.

4. Lichens.—The Lichen flora of the county is by no means exhausted. The subalpine and alpine species are those in which the county flora is naturally deficient, although singularly enough the gravel beach at Lydd, which is only a few feet above the sea level, furnishes a larger number of subalpine species than any other part of the county. Species which in Devon and Cornwall occur on the borders of Dartmoor are here scattered over small prostrate bushes which, dwarfed by the rough winds that sweep over the level ground of Romney Marsh, rarely reach more than a foot and a half in height.

The other groups that are not well represented are those which might be expected to occur on limestone walls and old ruins and on aged trees in parks. The limestone district of the Lower Greensand and the wooded districts alluded to under mosses, are those most likely to furnish species new to the county.

As a rule wooded districts a few miles from the sea are the richest for lichens, especially in damp valleys and on isolated trees exposed to the light and air. Oak trees in particular are furnished with a great number of species. Within a radius of about twenty miles from London, lichens are, as a rule, imperfectly developed, the smoke of towns being particularly detrimental to their growth. Where lichens are abundant, as a rule, the S.W. side of a tree is the richest, and the S.W. side of the county is likely to yield more than the eastern.

5. Marine Algae.—No list of the marine algae of the county has been published so far as I am aware. Mr J. T. Neeve of Deal has explored the neighbourhood of that town with remarkable success, having detected a species new to science, *Gonimophyllum Buffhami*, as yet found nowhere else, although the seaweed *Nitophyllum laceratum*, upon which it is a parasite, is quite a common species on the English coast. The neighbourhood of Folkestone

and Sandgate has been explored by Mr J. Cosmo Melvill and myself, but the rest of the coast is, I believe, entirely unexplored. Only about 110 species out of more than 750 British species have as yet been found in the county, although at least 500 of the British species might reasonably be expected to occur. The richest places are, as a rule, muddy estuaries, the open sea where streamlets run into it, *Zostera* beds, and rocks exposed at low water. Also places where two tides meet, as at Whitstable. The shells dredged by fishermen often have deep-water species attached to them.

The limit of the northern species on the east coast of England still needs accurate determination, one of these, *Monostroma Blyttii*, having been found as far south as Deal. On the western coast several of these have been traced as far south as Anglesea, but on the eastern coast, where the influence of the Gulf Stream is far less felt, they might be expected to extend further south, and the coast of Kent might furnish important data on this point.

That this group of plants affords an excellent field for work is, I think, evident from the fact that a small band of British algologists, less than half a dozen in number, have succeeded in nearly doubling the number of British species known in 1851, raising them from 400 to about 750 in 1898. This has been done, chiefly, by searching for the species known on the adjacent shores of Norway and France, but not in England. The majority of these have been found in England and Scotland, but several deep-water species yet remain to be discovered.

6. Fresh Water Algae.—A few local lists of fresh-water algae, including diatoms and desmids, have been published, but these only furnish a very small proportion of the possible number of species that should occur in Kent. The best localities for search are the brackish and fresh-water marsh ditches from Gravesend to Pegwell Bay, and those around Minster near Canterbury, also the ponds and ditches of the Weald and Gault, and the springs issuing from the Chalk hills. The military canal near Hythe and the ditches in Romney Marsh should afford many species. Spring and autumn are, as a rule, the best time to collect them. In summer the growth of aquatic plants is often so luxuriant that many species are hidden.

It is necessary also for the collector to remember that many species, such as *Spirogyra*, which are not easily determined except when in fructification, often assume, when in that state, a yellowish tint that would suggest decay and perhaps prevent their collection.

Hitherto I have spoken only of the work to be done in the county of Kent, but much of the same kind of work requires to be done in the other three counties.

I may perhaps take this opportunity to direct attention to

another class of work which badly needs careful workers. I allude to the life-history of Algae, both marine and fresh-water, but particularly the latter. We know but little of the changes that take place between the time that they disappear and reappear again. We do not even know whether some of the Algae are not merely stages of growth of others. This is especially true of those plants in which only vegetative growth and reproduction are known, and in which sexual reproduction is unknown. Anyone who would take the trouble to cultivate such plants as *Porphyridium*, *Chroococcus*, *Oscillaria*, *Tetraspora*, and *Schizogonium*, and reveal their life-history throughout the year, would add very considerably to our knowledge. This would not be a difficult task to those residing in the country who could check results obtained under cultivation by observations made in the localities where the plants flourish. Those who reside near the sea might attempt to solve some of the problems that are still attached to some of the commonest marine Algae. A microscopical examination once a month or once a fortnight during the year might result in finding the cystocarps of *Rhodymenia palmata*, the unknown tetraspores of some of the species of *Phyllophora*, and *Ahnfeltia plicata*, of *Gigartina mamillosa*, or of the rarer *Bonnemaisonia*, and *Sphaerococcus*, also the fructification of *Sphacelaria scoparia*, which, so far as is known, has never been found in this country.

The cultivation of the common Laminariae from spores might also throw some light on the life-history of these remarkable plants.

A list of the species of British Marine Algae in which certain forms of the fructification are unknown, is given in the *Annals of Botany*, vol. v., November 1891, No. xx.

Other branches of botanical work to which attention might be directed are:—

(1) The relation of the distribution of plants to water-sheds, geological formations, and chemical constituents of the soil, drainage, land carriage, agricultural seeds, and other causes.

(2) The special means by which different mosses rarely found in fructification are propagated and distributed.

(3) The rate of growth of different species of lichens.

(4) The relation of Hymenomycetes and Gasteromycetes, and other plants, to the roots of special trees or plants.

(5) The spread of parasitic fungi, injurious to plants, from wild plants to cultivated species of the same genus.

The list of the species of mosses and scale-mosses which should be looked for in Kent is here given. The list of the species of the Lichens and Fungi is not given, but only the names of the groups that are least represented, but plants of which are most likely to be

found, since it is impossible to state with any degree of certainty the species that might occur.

E. Morell Holmes.

Pharmaceutical Society's Museum.

APPENDIX

MOSSES

Sphagnaceae—	
Sphagnum subsecundum var. auriculatum	Bogs.
S. squarrosum var. teres	Bogs.
S. molle var. Mulleri	Bogs.
S. cuspidatum var. plumosum	Bogs.
S. strictum	Bogs.
S. laricinum	Bogs.
Polytrichaceae—	
Polytrichum gracile	Peaty heaths.
Buxbaumiaceae—	
Diphyscium foliosum	Shady sand rocks.
Dicranaceae—	
Seligeria recurvata	Sandstone boulders in damp woods.
Brachyodus trichodes	Sandstone boulders in damp woods.
Ceratodon conicus	Walls or banks.
Dicranum spurium	Boggy heaths.
Ditrichum tortile	Stone pits in the weald.
Campylopus brevipilus	Boggy heaths.
C. longipilus	Boggy heaths.
C. subulatus	Damp sandy roadsides.
Fissidentaceae—	
Fissidens rivularis	Under overhanging rocks in stream.
F. crassipes	Limestone walls near, or on stones in, streams.
Grimmiaceae—	
Grimmia orbicularis	Limestone walls.
Campylostelium saxicola	Shaded sandstone boulders.
Tortulaceae—	
Acaulon triquetrum	On the top of cliffs near the sea.
Barbula spadicea	Calcareous stones in or near streams.
Cinclidotus riparius	Sides of rivers on woodwork.
Pottia asperula	Ledges on sea-cliffs.
P. crinita	Ledges on sea-cliffs.
P. viridifolia	Ledges on sea-cliffs.
P. Wilsoni	Ledges on sea-cliffs.
Tortula atrovirens	Ledges on sea-cliffs.
T. angustata	High exposed banks.
T. canescens	Ledges on sea-cliffs.
T. ruraliformis	Sandy shores.
T. Vahliana	Damp chalky or calcareous banks.
Trichostomum tortuosum	Limestone banks and walls, in hilly districts.
Orthotrichaceae—	
Orthotrichum obtusifolium	Roots of trees.
O. pallens	Trunks of trees.
O. pulchellum	Trunks of trees.
O. pumilum	Trunks of trees.
O. Schimperi	Trunks of trees.
Funariaceae—	
Ephemerum cohaerens	Moist banks.
E. sessile	Fallow fields.
Funaria calcarea	Limestone banks.
F. microstoma	Damp depressions on heaths.

Bartramiaceae—

Bartramia stricta, . . .	Upland hedge banks.

Bryaceae—

Orthodontium gracile, . .	Rotten tree stumps and sand-rocks.
Webera Tozeri, ...	Damp clayey shady banks.
Bryum Warneum, . . .	Damp sandhills near the sea.
B. calophyllum, . . .	
B. Marrattii, . . .	
B. lacustre, . . .	
B. inclinatum, . . .	Walls.
B. uliginosum, . . .	Damp banks and bogs.
B. obconicum, . . .	Heaths.
Mnium riparium, . . .	Tree-stumps often submerged by streams.
M. subglobosum, . . .	Bogs and marshes.

Hypnaceae—

Cylindrothecium concinnum, .	Chalk hills.
Pylaisia polyantha, . . .	Tree trunks.
Brachythecium campestre, .	Damp stony fields and tree-roots.
B. salebrosum, .	
Eurhynchium speciosum, . .	Damp shaded stones and tree roots.
Amblystegium varium, . .	Damp trunks of trees.
A. Kochii, . .	Marshy meadows.
Hypnum imponens, . . .	On bare places on damp heaths.
H. giganteum, . . .	Bogs.

The names here given are those adopted in the Students' Handbook of British Mosses by Dixon and Jameson.

Lichens

The following genera are those which are but slightly represented as yet in the Kentish Flora, but what might be expected to afford new species for the county. At present 181 species, exclusive of varieties, have been recorded.

Collemei—
Collema.
Collemopsis.
Leptogium.
Caliciei—
Sphinctrina.
Trachylia.
Cladodei—
Boeomyces.
Cladonia.
Parmeliei—
Parmelia.
Squamaria.
Placodium.
Lecanorei—
Lecanora.
Lecideinei—
Lecidea.
Graphidiei—
Lithographa.
Graphis.
Opegrapha.
Arthonia.
Pyrenocarpei—
Endocarpon.
Verrucaria.

The majority of the genera mentioned are likely to afford only one or two new species for the county, but the genera *Lecanora*, *Lecidea*, *Arthonia*, and *Verrucaria* might afford a considerable number to a careful observer.

Scalemosses

Lejeunia Mackaii, ...	Tree trunks.
L. calyptrifolia, ...	Furze stems and rocks.
Porella pinnata, ...	Trunks and stones occasionally submerged.
Ptilidium ciliare, ...	Heaths.
Trichocolea tomentella, ...	Damp woods.
Cephalozia catenulata, ...	Sand rocks.
C. multiflora, ...	Shady banks or woods.
C. curvifolia, ...	Damp rotting prostrate trunks.
C. Francisci, ...	Damp heaths.
C. fluitans, ...	Bogs.
C. Sphagni, ...	Or Sphagnum.
C. denudata, ...	Damp sand-rocks.
C. Turneri, ...	Ditch banks or loamy or clayey sand.
Scapania resupinata, ...	Shaded rocks.
Aplozia gracillima, ...	Damp loamy banks.
A. lanceolata, ...	Dripping sand-rocks.

Aplozia riparia,	On the sides of subalpine streams.
Jungermannia lycopodioides,	Clayey banks.
J. baibata	Shady banks.
J. porphyroleuca,	On damp mossy banks.
J. bicrenata,	On banks in woods.
Saccogyna viticulosa,	On damp shady banks of cuttings.
Nardia adusta,	Shaded sandstone rocks.
N. Funckii,	On heathy soil or stony woods.
N. hyalina,	Growing in sphagnum.
Fossombronia caespitiformis,	Damp ground in fields or woods.
Petalophyllum Ralfsii,	In damp hollows of sand dunes.
Pallavicinia Lyellii,	Dripping sand rocks.
Aneuria palmata,	On dead trees in damp woods.
Aneuria latifrons,	On naked ground near streams.
Dumortiera irrigua,	On shaded banks of streams.
Targionia hypophylla,	On earthy ledges of rocky hedge banks.
Ricciocarpus natans,	Floating in marsh ditches.
Sphaerocarpus terrestris,	In clover fields.

The names here given are those adopted in Cooke's "Handbook of British Hepaticae." Most of the species mentioned are likely to occur in the S.W. or central parts of the county in wooded hilly districts, especially where Greensand rock or clayey sand occurs.

FUNGI

Hymenomycetes:
- Auricularini—
  - *Cyphella.*
- Clavariei—
  - *Typhula.*
  - *Pistillaria.*

Gasteromycetes—
- *Octaviana.*
- *Melanogaster.*
- *Hymenogaster.*

Myxogastres—
- *Diderma.*
- *Didymium.*
- *Physarum.*
- *Badhamia.*
- *Stemonitis.*
- *Trichia.*
- *Licea.*

Coniomycetes—
- *Phoma.*
- *Leptothyrium.*
- *Sphaeronema.*
- *Sphaeropsis.*
- *Diplodia.*
- *Hendersonia.*
- *Vermicularia.*
- *Septoria.*
- *Excipula.*
- *Asteroma.*
- *Discella.*
- *Coryneum.*
- *Myxosporium.*
- *Glocosporium.*
- *Sporidesmium.*
- *Puccinia.*
- *Podisoma.*
- *Trichobasis.*
- *Uromyces.*
- *Lecythea.*
- *Ustilago.*

Hyphomycetes—
- *Isaria.*
- *Pachnocybe.*
- *Stilbum.*
- *Tubercularia.*
- *Fusarium.*
- *Dendryphium.*
- *Helminthosporium.*
- *Cladosporium.*
- *Dactylium.*
- *Sporotrichum.*
- *Fusisporium.*

Ascomycetes—
- *Peziza.*
- *Tympanis.*
- *Cenangium.*
- *Stictis.*
- *Ascomyces.*

Tuberacei, all the genera—
- *Rhytisma.*
- *Hysterium.*
- *Hypocrea.*
- *Hyponyl.*
- *Valsa.*
- *Dothidea.*
- *Stigmatea.*
- *Nectria.*
- *Sphaeria.*
- *Erysiphe.*

The names here given are those adopted in Cooke's "Handbook of British Fungi." An excellent monograph of the British Gasteromycetes, by Mr Geo. Massee, has been published in the *Annals of Botany*, vol. iv., pp. 1-103.

III

The Goldfish and other Ornamental Fish of Japan

THE goldfish or *Kingyo* is supposed to have been introduced to Japan from China; but the Japanese varieties, so far as I know, differ greatly from those now found in China, so that the introduction of this pretty fish into Japan, if it really was introduced, must have been effected in a very remote past.

The goldfish is a favourite ornamental fish throughout the empire of Japan. There are many large culture ponds in the warmer part of the empire. Famous places for the culture of goldfish are Tōkyō, Ōsaka, and Kōriyama. The most beautiful fancy fish may be found in Tōkyō and Ōsaka, usually in the aquaria of amateurs. Great pains is taken to select those which have beautiful colours, pretty or singular forms, and graceful motion. Of course, differences of taste govern the selection in different localities and in different times. However the general principles of selection are fairly constant, and I am inclined to believe that the taste of Ōsaka is always best. Now I shall state the chief characters that qualify a fish to be regarded as choice, and then shall give short descriptions of the principal varieties of goldfish found in Japan.

A choice fish should possess the following characters:—the lips, nostrils, circumference of the eyes, operculum, and fins ought to have colours, *i.e.*, people wish to have fish the extreme parts of which are all coloured, the remaining portions of the body may remain colourless; but when small colour-spots are evenly distributed over the body, when the hinder portion of the body is coloured, or when the head is coloured, the fish is thought to be much more beautiful. As for the colour of the fins the deeper it is the better.

The fins ought to be large, delicate, but rather stiff, not falling into folds like a withered flower. Moreover they ought not to prevent the free locomotion of the fish.

The caudal fin should be three-pointed, *i.e.*, somewhat triangular in shape or lozenge-shaped, not divided at the median line. It should be well expanded and rather erect. The anal fin ought to be laterally divided into two lateral equal portions.

The movement must be graceful. A fish which cannot keep its longitudinal axis of the body horizontal is considered inferior. The body should be plump and have an outline of beautiful curves. And the fish must be healthy. The fish represented in Figs. 1 and 2 are very fine, while those represented in Figs. 3 and 4 are common and inferior.

The variety which is considered to be most graceful is known by

the name of *Maruko, Chōsen,* or *Ranchū* (Fig. 1). The body is short, round, or sometimes ovoid. It is not compressed laterally, the dorsal median part being flat. Scales few and irregular in number, and large in size. The head is large, short and round, and sometimes has many warts on it, as in the following variety. The dorsal

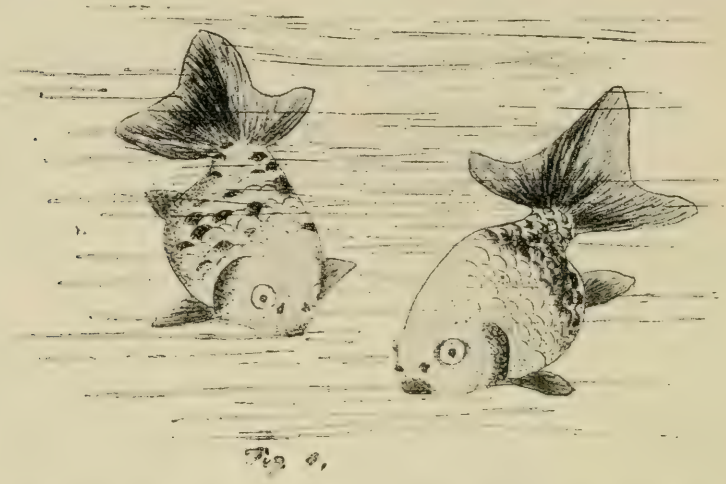

Fig. 1.

fin is wanting, while the caudal fin is very large. The eyes are also large. This variety does not attain a large size, seldom exceeding six inches in the total length. It is very weak, so that great care is required for its culture.

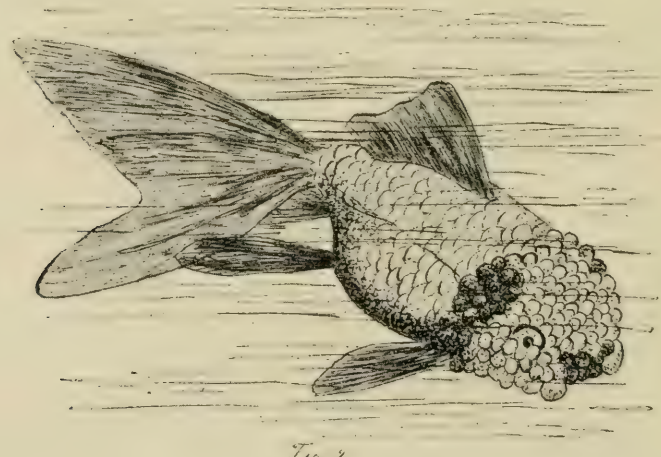

Fig. 2.

Next in beauty and fancy is the variety known by the name of *Shishigashira, Onaga,* or *Oranda* (Fig. 2). This variety is characterised by the short and ovoid body, the presence of many warts on the head, and enormous development of the fins. The caudal fin is especially well-developed, being longer than the body. It is

said that this variety was produced about fifty years ago in Ōsaka by crossing the preceding variety with the next variety. The fish of this variety attains the length of about one foot. It is more hardy than the preceding variety and is easy to keep. There is a subvariety called *Hiroshima*, the peculiarity of which is the presence of a large prominent wart on each side of the snout.

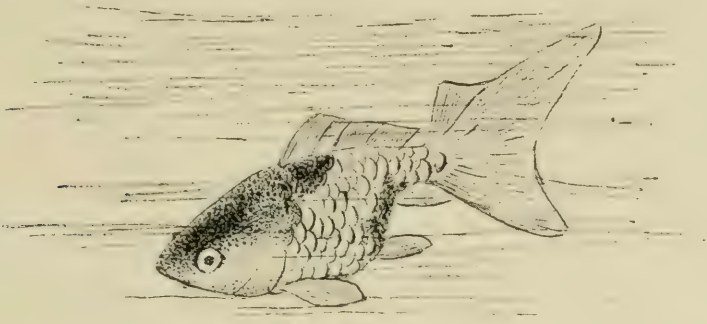

Fig. 3.

When special attention is not paid to the rearing of the fish, warts do not come out at all.

Next comes the variety called *Rūkin* or *Nagasaki* (Fig. 3). The body is elongated and laterally compressed, the head pointed, the caudal fin very large, the other fins normal in size, and the anal fin generally paired. This variety does not attain a large size, only

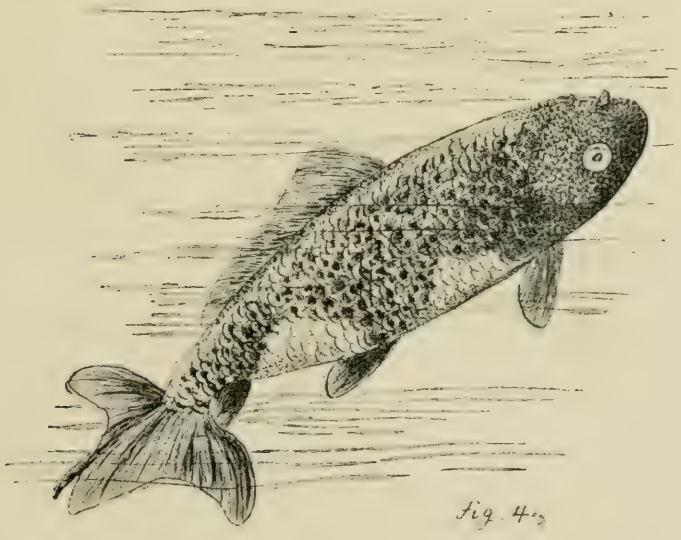

Fig. 4.

about the size of *Maruko* (Variety 1). But it is very hardy. It is not so much esteemed as the preceding two varieties, and consequently small pains are taken in its selection.

The fourth variety is called *Wakin*. It is the common goldfish,

the least specialized form (Fig. 4). The body is very much elongated and compressed laterally, the scales are small and the fins normal. The anal fin is sometimes paired, sometimes not. The caudal fin sometimes is not divided laterally. This is the most hardy variety, and attains a length of one foot or more.

The above-mentioned four varieties are the principal kinds of goldfish in Japan. Of course there are many intermediate forms and subvarieties. The colours of the fish are generally crimson, red, vermilion, yellowish, and golden-yellow. Sometimes we find fish with the colour and lustre of iron.

The so-called 'Telescope-fish' is not a Japanese variety, but was introduced from China, after the war with that country.

As the coloured markings in the goldfish are considered as the most important element of beauty, some culturists invented a way of bleaching some parts of the coloured portion and so increasing the beauty of the fish. This is done by the application of a fine brush, soaked in a dilute solution of a chloride or chlorides, to those surfaces of the body that they wish to bleach. This must be done after completely absorbing the moisture from the spot. By this method you may obtain fish with signs, letters, or characters bleached out in the coloured portion of its body.

To keep choice goldfish large aquaria or ponds are necessary. Small aquaria, running water, and cold water are not good for goldfish. To keep a pair of the adult goldfish, an aquarium should contain at least eight gallons of water.

Besides the goldfish, the goldcarp, the silver-cheeked carp, and the golden *Medaka* are also reared as ornamental fish.

The goldcarp or *Higoi* is generally kept in large ponds. It is very hardy and attains a length of two or three feet. There are different colours in this fish: brown, golden yellow, vermilion, pinkish, white, or variegated with black and red spots. This is a variety of the common carp, and in Japan it almost always forms a proportion of the embryos hatched out from the spawn of the latter. The flesh of the goldcarp is far inferior to that of the common carp and is not good for food. Though the goldcarp prefers rather muddy and still water, it thrives also in clear water.

The silver-cheeked carp or *Hōkin* is also a variety of the common carp. It is a very pretty fish, brown or greyish in colour, and has the cheeks with silver-like lustre. It does not attain a large size, being generally less than one foot in total length. This variety is not common, and is found only in Kōriyama.

The golden *Medaka* belongs to a variety of *Fundulus sp.*, and is only about one inch in total length. It is generally yellowish or light vermilion in colour, and being hardy is suitable for keeping in small aquaria as a children's pet.

K. KISHINOUYE.

IMPERIAL FISHERIES BUREAU, TŌKYŌ.

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IV

The Progress of Research on the Reproduction of the Rotifera

OF the numerous problems presented by the Rotifera, none are more important than those connected with the complex reproductive relations of these animals. The extreme degree of sexual dimorphism, and the prevalence of parthenogenesis to the probable exclusion, in some cases, of sexual reproduction, are striking features which, while not without parallel in other groups, can nowhere be more conveniently studied. In spite however of the great amount of attention which has been directed to the group, many points in their life-history are still obscure, while some of the most fundamental facts have only recently been definitely ascertained.

In Ehrenberg's great work on the Infusoria (1), from which our exact knowledge of the group may be said to date, the Rotifera are described as hermaphrodite, the convoluted excretory tubules having been mistaken for the testes and their ducts. While it was soon recognised by other observers that these structures had nothing to do with reproduction, the view that the rotifers were hermaphrodite appeared to be confirmed by Kölliker's (2) discovery of spermatozoa within the body-cavity of *Megalotrocha*. Since the ovary and oviduct are completely shut off from the body-cavity, it seemed obvious that these spermatozoa must have originated where they were found, and indeed Kölliker described them as developing from nucleated cells in the body-cavity. The first known male rotifer was described in 1848 by Brightwell (3) in the species afterwards named in his honour *Asplanchna brightwelli*. In the following year the same species was made the subject of a careful monograph by Dalrymple (4), who recognised the complete absence of the alimentary system in the male. In 1857 P. H. Gosse, in a well-known paper (7), described the males of ten species and indicated their probable existence in several other forms belonging to distinct families of the Rotifera. He affirmed the dioecious character of the group as a whole, and compared the degraded anenterous males with those of the Cirripedia which had then recently been described by Darwin. In 1897 C. F. Rousselet (27) gave a list of nearly one hundred species in which the male forms were known, and the number has since been added to by Weber (29) and others.

While in the great majority of these cases the male differs from the female not only by his much smaller size but also in the want of an alimentary canal and of the characteristic rotiferan mastax, four species are now known where this difference in structure between the two sexes does not exist. In the very aberrant genera *Scison* (9) and *Parascison* (14), which live as ectoparasites on *Nebalia*, the male differs from the female only in the reproductive organs. To these have recently been added the cases of *Rhinops vitrea* (Rousselet 27) and *Notommata wernecki* (Rothert 26, Rousselet 28), the latter being also a parasitic form living in curious gall-like excrescences on the alga *Vaucheria*. In the last-named rotifer the males are only to be distinguished from young females by very careful examination. While males are now known in very many genera belonging to nearly all the families of the Rotifera, a notable exception occurs in the case of the family Philodinidae, of which, as yet, only females are known. Mr Rousselet (27) suggests that possibly, as in the case of *Notommata wernecki*, the males of this family may have been overlooked from their resemblance to the females. He notes that 'resting-eggs' (the association of which with the occurrence of males will be referred to below) have been identified with more or less certainty by Janson (22) and Bryce in one or two species of *Callidina*. It must be remembered however that the *Philodinidae* have developed to the highest degree a method of resisting drought alternative to that afforded by the resting-eggs, namely, the encystment of the adult animals, and also that exclusively parthenogenetic reproduction is not unknown in other groups of animals (*e.g.*, Ostracoda and Cladocera). It is, therefore, by no means impossible that the male sex may really be non-existent in many of the species composing this family. Of the difficulty attending the search for male rotifers we have a striking example in the case of *Stephanoceros*, where the recent discovery of the male (25) followed at an interval of not less than 130 years that of the familiar and conspicuous female.

The great activity of the male rotifers as compared with the females is a characteristic feature, not only in those forms where the females are sedentary (Rhizota), but even in the case of the most powerful swimmers among the Ploima. In *Brachionus*, Hudson and Gosse (13) describe the male as leading "a brief life of restless energy, now darting from place to place so swiftly that the eye can scarcely follow it, and now whirling round as if anchored by its curved foot and penis. It often circles round the female, attaching itself now here, now there, and forcing its companion to waltz round and round with it, from the top of the phial to the bottom." In these circumstances it is a matter of great difficulty to observe the actual process of coition. Brightwell only speaks of seeing the

male *Asplanchna* "attached to the side of the female," and though Dalrymple refers to the "intromission of the male organ into the vaginal canal," it would appear that this was rather an inference than an observation. Gosse however observed and described the process of impregnation by the cloaca in *Brachionus*. Cohn (6) observed in *Hydatina* the males adhering to any part of the body of the females. He found spermatozoa in the body-cavity of the latter, and recognizing the improbability of their having reached that position by way of the cloaca, he was led to suspect the existence of a special copulatory pore in the region of the neck. He was unable, however, to demonstrate any such aperture. In 1885, Plate (12), investigating the same species, stated that impregnation took place by the penis of the male perforating at any point the body-wall of the female and injecting the spermatozoa into the body-cavity. A similar process had previously been described by Lang (11) in certain Polyclad Turbellarians; and it is now known to occur in several other groups of 'worms,' notably in certain Hirudinea where it has been investigated by Whitman (20). The last-named writer names the phenomenon "hypodermic impregnation," and gives a full summary of previous observations on its occurrence in other groups.[1] M'Murrich has since adduced evidence to show that it occurs even in certain Isopod Crustacea. Plate's account, though doubted by so great an authority on the Rotifera as Dr Hudson, has been confirmed by Maupas, whose important researches will be referred to below. It is not yet clear, however, in what way the unarmed penis of the male can perforate the tolerably resistant cuticle of the female; nor can it be doubted that hypodermic impregnation is by no means universal among the Rotifera, for cloacal coition has been seen by many observers, and has been figured by Weber (15) in *Diglena catellina*.

In several species, Ehrenberg described, besides the more usual thin-shelled 'summer' eggs, thick-shelled 'winter' eggs, which only hatched after a long resting-period. Huxley, studying these in *Lacinularia* (5), applied to them the name 'ephippial' eggs on the analogy of the similar structures so named among the *Cladocera*. He, however, fell into the error of regarding them as multicellular buds like the 'gemmules' of sponges or the 'statoblasts' of Polyzoa. Cohn was the first to offer what has since been the generally accepted interpretation of these two kinds of eggs. He observed that the production of 'winter-' or, as he preferred to call them, 'resting-eggs,' was always associated with the appearance

[1] Although Prof. Whitman's paper is entitled "Spermatophores as a means of hypodermic impregnation," it does not appear that spermatophores are formed in *Hydatina*, the only rotifer in which the process has been observed with certainty. So far as we know, such structures have only been observed in *Paraseison* (Plate, 14) and in *Asplanchna helvetica* (Masius, 18).

of males, and he therefore concluded that the 'resting-eggs' were produced after fertilisation, while the 'summer-eggs' (distinguishable by a difference in size into male and female eggs) were parthenogenetic. Partly from the fact that very similar phenomena were known in other groups, especially in the Cladocera, these views obtained general acceptance, despite the fact that in his later papers (8) their author found it necessary to point out some difficulties. It had already been observed that each individual female produced only one variety of eggs, male, female, or 'resting,' during her lifetime, and Cohn found spermatozoa in the bodies of females which were laying male or female 'summer' eggs, as well as in those laying 'resting' eggs. Joliet, in his monograph on *Melicerta* (10), corroborated these observations, and after giving a careful summary of the evidence, suggested that the facts might be explained by supposing that the sexually produced eggs hatched into females which laid 'resting' eggs. In 1885, Plate (12), in connection with his discovery of 'hypodermic impregnation,' arrived at the strange conclusion that impregnation was always abortive and without influence on the eggs. He contradicted Cohn's statement that the spermatozoa in the body-cavity of the female were attracted to the neighbourhood of the ovary, but found, on the contrary, that they disintegrated within a few hours after being introduced, without making any progress towards reaching the ovary. By observing isolated specimens he claimed to show that impregnation was without effect, male, female, and resting-eggs being laid indifferently by impregnated and virgin females. His explicit statement that he obtained resting-eggs from a female isolated from birth is in direct conflict with the results of later investigations. Plate concluded that uninterrupted parthenogenesis prevailed among the Rotifera, that the males were a vestigial and superfluous sex, and that the abortive attempts at impregnation were an atavistic phenomenon without present significance in the life-history of the species. In 1890, Maupas (16, 17, 19), attacked the subject by applying to the rotifers the methods of culture used by him in his classical researches on the reproduction of the Infusoria. Unfortunately only very brief accounts of his laborious experiments have been published, and many details of his procedure are not explained. He found in the species chiefly studied by him (*Hydatina senta*) two varieties of females, distinguished only by the eggs which they produced, one kind laying eggs which gave rise to females, the other laying only male eggs.[1] The females of the latter variety alone were capable of being successfully fertilised, and that

[1] It would be difficult to find an exact parallel to the curious 'sub-sexual' difference between these two classes of females. That it is not accompanied by any conspicuous difference of size or structure is certain, but it would be interesting to know whether careful measurements would not reveal a physical dimorphism correlated with the physiological one.

only if fertilisation took place at a very early age before they had begun to lay eggs. On older females or on those capable of producing female eggs, impregnation, if it took place, was without result. When fertilisation was successfully accomplished, however, the individual produced only resting-eggs throughout life. This delimitation of the conditions necessary for successful fertilisation explained the older observations of spermatozoa within females laying other than resting-eggs, as well as Plate's failure to trace the history of the spermatozoa within the body of the female. The evidence as to the fertilisation of the resting-eggs has been completed by the observations of Sadones (31), who observed the passage of spermatozoa through the ovarian wall in *Hydatina*, and of Lauterborn and v. Erlanger (32), who have traced the fusion of the male and female pronuclei in the resting-eggs of *Asplanchna*. Nussbaum (30) was unable to satisfy himself of the correctness of Maupas' view that fertilisation only affected eggs which would otherwise have given rise to males, but confirmatory evidence on this point is afforded by Lauterborn's observations on *Asplanchna*. In this viviparous form, Masius (18) had already noted the occurrence of a resting-egg in the oviduct along with ordinary parthenogenetic eggs or embryos, and Lauterborn, confirming this, finds the latter to be always males. The fact that in *Asplanchna* the same individual may produce both 'resting' and parthenogenetic eggs (as is perhaps also the case with *Synchaeta* [Apstein] and *Notommata werneckii* [Rothert]), indicates that in this respect Maupas' results as to *Hydatina* are not to be extended without qualification to other forms. The rule that male and female eggs are produced by different individuals may also be not without exceptions, especially in the genus *Brachionus*, where Daday (*fide* Weber, 15) and Wesenberg-Lund (33) have observed both kinds of eggs carried on the carapace of the same individual. In this connection it may be remarked that the difference in size between the two sexes, and therefore between the eggs producing them, is not nearly so marked in *Hydatina*, for instance, as in such forms as *Brachionus;* and that in the former genus Nussbaum and others have observed a continuous gradation of sizes between the two, so that in some cases it was impossible to determine the sex of an egg without observing the development of the contained embryo. It is possible that want of sufficient attention to this point may have led to some of the discrepant results mentioned above. Of course, when the embryos have reached a certain stage, the presence or absence of the mastax at once indicates their sex.

The question of the causes which lead to the appearance of males and the consequent production of resting-eggs is one of great interest, and has received several answers, none of which can yet be regarded as quite satisfactory. It has long been known that the

occurrence of resting-eggs is not confined to any one season of the year, though the misleading term 'winter-eggs,' or even explicit statements of the erroneous view implied by it, are still current in text-books. Joliet (10) observed that the males and resting-eggs of *Melicerta* were much more plentiful in dry than in wet seasons. Maupas (19) reached the conclusion that temperature was the determining factor in the case of *Hydatina*, and that the sex of the offspring was determined two generations in advance. He states with great confidence the view that it is at the moment when the ovum is differentiated in the ovary that the temperature influence determines whether it shall develop into a male-producing or a female-producing individual. His experiments showed that while the proportion of male-producing females hatched from eggs laid at a temperature of 14°-15°C. varied from 5-24% of the total, it rose to 81-100% when the temperature was increased to 26-28°C. Against this, Bergendal (21) and Wesenberg-Lund (33) have recorded the occurrence in Greenland and Denmark respectively of different species of male rotifers in water at a temperature little above zero, while Lauterborn points out that the males of most species are conspicuously absent during the warmest months of the year. Nussbaum (30) has recently subjected Maupas' results to a detailed criticism and considers them inconclusive. Working by similar culture-methods, he claims to have shown that nutrition is the determining factor. He found that females of *Hydatina* insufficiently fed during the early part of their life afterwards laid only male eggs, while well-nourished individuals produced female eggs. This is obviously in harmony with other well-established instances of the influence of nutrition in the determination of sex.

Lauterborn (32), studying the seasonal variations of pelagic Rotifera in the lakes and ponds of the Upper Rhine district, comes to a conclusion on this point differing both from those of Maupas and of Nussbaum. He believes that the appearance of males and the consequent production of resting-eggs, in a word, the sexual period, is normally recurrent in the life-cycle of each species, after a certain definite number of parthenogenetic generations, and is only secondarily modified by external conditions, especially in the case of those species which have left the relatively stable environment of the large lakes for a precarious existence in pools and puddles. In this connection he makes an ingenious suggestion which tends to reconcile the conflicting results of Maupas and Nussbaum, while depriving them of their universal application. Both these observers studied *Hydatina senta*, a species which has supplied the *corpus vile* for very many investigations since the days of Ehrenberg. Now *Hydatina* is characteristically an inhabitant of small accumulations of water, wayside puddles and the like, where it feeds on the *Euglenae*

and similar organisms which swarm in such situations. It is thus very liable to be killed by drought, since it does not possess the power of withstanding drying exhibited to a marked degree by certain rotifers. But the rise of temperature preceding and causing the drying up of the water may not only act directly on the *Hydatina* as Maupas believes, determining the appearance of males and of resting-eggs, but it also brings into play the factor emphasised by Nussbaum by diminishing the supply of food, for the *Euglenae* encyst themselves and become aggregated into masses, in which state they cannot be ingested by the rotifers. Lauterborn considers that in *Hydatina*, and probably also in other species of similar habits, the power of ready response to changes in these two factors of its environment is a special adaptation ensuring the production of resting-eggs when the colony is threatened with extermination by the drying up of its habitat. It has already been pointed out that in the Philodinidae, some of which live in a minimal quantity of water among damp moss or earth, the same end has been reached in another way, the adult animals becoming encysted in an envelope of hardened mucus, and in this state exhibiting an almost incredible power of resistance to prolonged drying.

Lauterborn finds that the pelagic Rotifera may be divided into three categories :—

(1) Perennial forms, which occur in greater or less abundance all the year round.

(2) Summer forms, which are found only in the summer months.

(3) Winter forms, including a few species whose occurrence is limited to the colder months of the year.

The species belonging to the last two classes he finds to be strictly 'monocyclic,' in the sense in which Weismann has applied that term among the Cladocera; that is to say, the sexual period occurs only once a year, in autumn in the case of summer forms, in spring in the winter forms, when resting-eggs are produced to tide over the unfavourable seasons of winter and summer respectively. The 'perennial' species, on the other hand, are di- or poly- cyclic, the sexual period occurring at least twice in the year, in spring and in autumn, but without any interruption of the process of parthenogenetic reproduction, which goes on all the year round, so that the species never disappears from the gatherings. It is pointed out that those species which, in the pelagic fauna of lakes, show this perennial character, are for the most part forms known to occur also in smaller ponds and pools, while the monocyclic summer and winter forms are largely truly pelagic (limnetic) species inhabiting the open waters of larger ponds and lakes. These results show considerable agreement with those obtained by Weismann among the *Cladocera*.

The observations of Wesenberg-Lund (33) on the Rotifera of Denmark, lead him to adopt Nussbaum's view that nutrition is the determining factor in the production of males. The chief result of his researches is to show that, immediately before the normal period of sexual reproduction, a period of very rapid parthenogenetic multiplication sets in, and it is when this has reached its height that males appear. Whenever it was seen that any one species was becoming predominant in the gatherings from one particular source, the approach of the sexual period for that species could be predicted. He finds, however, that, with the same species of rotifer, the sexual period may occur at different times even in neighbouring ponds. Lauterborn, on the other hand, states that the appearance of males of the same species in very different localities, from lakes to puddles, was often striking in its simultaneity.

Though only indirectly connected with the present subject, we may call attention in closing to some of the unexplained anomalies in the distribution of the Rotifera. While the great majority are notoriously sporadic and uncertain in their occurrence in any one locality, yet Lauterborn and others have recorded cases where the same species has appeared year after year in the same pool while absent from all the surrounding localities. Very many species seem to have a literally world-wide distribution, and their number is constantly being increased; yet there are instances of curiously restricted range, and this in cases where insufficient search cannot be adduced to explain their absence from certain areas. Thus *Lacinularia* is common in many localities in England; it swarms in every lough in certain districts in the west of Ireland, appearing in July, as Mr Hood (24) says, "as precisely to its season as the lapwing or the swallow," and yet we are assured by the same excellent authority that it is entirely absent from Scotland. Until 1883 the same appeared to be the case with *Stephanoceros;* since then, Mr Hood tells us, he has traced the gradual extension of its range until it has become by no means rare in the lochs of Forfarshire and Perthshire.

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University College, Dundee.

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SOME NEW BOOKS

Huxley as Student

The Scientific Memoirs of Thomas Henry Huxley. Edited by Prof. Michael Foster and by Prof. E. Ray Lankester. Vol. i., 8vo, pp. xvi+606, with frontispiece and 32 plates. London: Macmillan & Co. New York: Appleton & Co., 1898. Price, 25s. net.

It is always an interesting, though perhaps not a very important question, what is the true position of any one scientific worker. Some there are who attain a wide popularity, but whose names are almost unknown to, or passed over with a sneer by, the professional student of science. There are others whose names, though held in honour by their fellow-workers, are absolutely unknown, not merely to the general public, but to those circles of it that claim the epithet 'cultured.' It is rarely that a man of science receives adequate honour from both his fellow-workers and the great public; and in the one case or the other the honour, even if it comes, is pretty sure to be based on something else than true appreciation. Darwin, to mention the only name that will not be considered invidious,—Darwin was estimated by his fellow-workers at something approaching his true value, but to the outer public he was the man who said we were descended from monkeys, and little more. As such, his fame was enormous, but hardly desirable.

Of what nature is the reputation of Huxley? That he had a popular reputation is undeniable. Whether as the militant critic of orthodoxy, the writer of widely used text-books and professor in an important school, or as the lucid and polished exponent of a new scientific philosophy, he had his following both in this country and on the continents of Europe and America. But a reputation based on these forms of intellectual activity would hardly last were it not supported by a bulk of more solid scientific work, capable of winning the admiration of competent critics. A wide popularity is often looked at askance by scientific men, who regard it as incompatible with serious work. Often they are right, especially when meaner intellects are concerned; and even in the case of Huxley there might be some danger lest his brilliant essays should eclipse, even in the eyes of scientific workers, the sober background of original research. From this point of view, therefore, the point of view of memorial, to keep the example of Huxley before future generations, it was a wise thought to republish in collected form his more technical papers. Many of these were, in fact, issued in publications not easy of access by the general zoologist of to-day, such as *The British and Foreign Medico-Chirurgical Review*, Todd's *Cyclopaedia of Anatomy and Physiology*, and *The London Medical Gazette*, so that their republication in convenient form were in any case something to be grateful for.

We are not among those, if any there be, who need to be convinced of Huxley's greatness as a biologist. But just because we admit this so fully we are glad to have his articles thus collected in a strict chronological order, without addition or alteration, so that we may trace through them the modifications of view and the gradual

fructification of ideas. To take but a single example, it is of interest to read Huxley's expression of his views in 1855 as to the progressive development of animal life in time, and to contrast it with the views expressed by him thirty or forty years later, after he had been brought into almost daily contact with the concrete facts of palaeontology. Over and over again throughout the present volume, the first in a set of four, we see the agnostic spirit in which Huxley approached every problem and every statement by authority, such, for instance, as the teleological view of anatomical correlation enunciated by Cuvier.

Further, as one turns over these handsome pages, one cannot but remark on the diversity of subjects therein discussed. We find articles dealing with *Amphioxus*, Mollusca, Hydrozoa, Tunicata, echinoderms, Rotifera, parasitic and other worms, Brachiopoda, Protozoa, Crustacea, Bryozoa, human anatomy, fossil fish, fossil reptiles, and, incidentally, other groups of animals; or, taking the subject-matter from another stand-point, we find articles on histology, on general anatomy, on classification, on palaeontology, on embryology, on the cell theory, on types of structure, on glacier ice, on the problem of individuality, descriptions of new species, a cyclopaedia article on tegumentary organs, and a Croonian lecture on the vertebrate skull. All this was written before Huxley had reached the age of thirty-five. Is there any biologist now living who, when he was thirty-five, had produced work so considerable in bulk, so varied in its scope, and withal so searching and profound?

The editors and publishers have done their work well. The original place of publication, with sufficiently full bibliographical details, is quoted for each of the fifty articles herein contained. The reprinting, so far as we have tested it, appears to be exact, even down to the absurdly small type which happened to be used for certain catalogue numbers in a paper originally published by the Geological Society. The plates appear to have been re-lithographed, since they are not perfectly absolute facsimiles. They bear consecutive numbers, placed within square brackets, and each also has the number which it bore when originally published. The name of the original artist has been reproduced, as a rule, but has been accidentally omitted in one or two cases. In a few cases also, the reference number to the page which the plate is intended to face is incorrect. The name of the artist, engraver, or lithographer, who has reproduced the plates, does not appear to be mentioned. We note, however, that it is to Messrs Walker and Boutall that we are indebted for an excellent photogravure of the same photograph of Huxley as that reproduced in *Natural Science* for August 1895 (vol. vii., plate xviii.). The volume has been excellently printed by Messrs Richard Clay & Sons. We note that the publishers have undertaken all the financial responsibilities of this republication, and we hardly think they will have any occasion to regret their generosity. To them and the editors we offer our congratulations and thanks.

Sedgwick's Text-Book of Zoology

A Student's Text-Book of Zoology. By Adam Sedgwick. Vol. i. 8vo, pp. xii + 620. London: S. Sonnenschein & Co. 1898. Price, 18s.

Of late years the English student has had to rely for his guidance in the study of zoology almost entirely on translations of foreign works,

the supply of which has always been abundant. But a growing need was felt for a general and elementary 'Zoology' written more particularly so as to express the views of the majority of teachers in this country. The differences between the teaching and current doctrines in one country and those in another are not great, and perhaps not very important, yet they are distinctly appreciable. Mr Sedgwick has now brought out the text-book of general and systematic zoology of which the first volume is before us. In his preface the author tells us that it was his original intention to issue a revised edition of Claus' well-known 'Lehrbuch'; but that he changed his plan, and has written a book which is a new work although following the same lines as the older one.

The present text-book is intended to help the student who has already acquired a preliminary knowledge of certain types of animal life, and wishes to proceed to a more thorough study of zoology. The whole work will probably be completed in two volumes. The first, with which we are now concerned, deals with the Phyla Protozoa, Porifera, Coelentera, Platyhelminthes, Nemertea, Nematelminthes, Rotifera, Mollusca, Annelida, Sipunculoidea, Priapuloidea, Phoronidea, Polyzoa, Brachiopoda, and Chaetognatha. The second volume will include the Arthropoda, Echinoderma, and Vertebrata, with general discussions of "the facts and principles of Zoology."

The work is distinguished by the clear and straightforward manner in which it is written; whether we agree with the author or not, we never have any doubt as to his meaning. Superfluities are rigidly excluded, and by the judicious use of small type the bulk of the volume is kept within reasonable bounds. Throughout, the facts are treated in strictly systematic order, as many families are mentioned as possible, and a large number of genera are named. The book will, therefore, prove a very useful work of reference, in which both facts and names can be readily found—a task rendered easy by the help of an excellent index at the end of the volume.

As we should expect from the pen of an author whose researches have extended over such a wide field, and whose experience in teaching is so great, this volume is well up to date, and singularly free from those blunders which so frequently disfigure text-books. This is no doubt partly due to the fact that Mr Sedgwick has often secured the help of specialists in reading over and correcting the proofs.

Mr Sedgwick, as is well known, is an opponent of the 'cell-theory' —accordingly in his general definitions he endeavours to avoid the use of its 'language.' For him, the Metazoa are merely multinuclear animals, "in which the nuclei are for the most part arranged regularly and with a definite relation to the functional tissues." We must confess that this attempt to boycott the 'cell-theory,' which after all is merely a descriptive statement of perfectly obvious facts, tends to become somewhat pedantic, and moreover is futile, since again and again the writer is obliged to return to "the language of the cell-theory," without which no description of Metazoan tissues can be intelligible or correct. If certain authors have erroneously taught that the cells of a tissue or embryo are entirely independent organisms, surely this is no reason why we should abandon the use of the term cell, any more than we should give up the words 'segment' or 'meta-

mere,' because some authorities have considered these parts to represent distinct individuals in an animal colony.

This volume contains a vast amount of information, yet it is inevitable that the various groups should be treated somewhat unequally. The parts on the Porifera and Coelentera, for instance, are rather dry. Opportunities are lost for interesting comparisons. With regard to the sponges, no discussion of affinities is given, the importance of the collar-cells and of the development is not sufficiently brought out; within the group itself, the significance of the Olynthus stage of development as representing a possible ancestral condition is not appreciated. Similarly, the relationships of the various Coelenterate groups are not discussed, and the resemblances between the Acalephae and Actinozoa (Anthozoa) seem to us scarcely enough insisted upon. No adequate discussion of the affinities of the Ctenophora can be found, they are placed in the Phylum Coelentera, and a reader of this book might never suspect that many authorities separate them entirely from the Coelenterates. *Coeloplana* and *Ctenoplana* are merely mentioned.

The Platyhelminths, Nemertines, Nematelminths, and Rotifers are placed outside the true Coelomata. A full discussion of the homology of the coelom is promised in the general part of the next volume; for the present we are given the dogmatic statement that "the coelom is derived from the enteron," a statement which cannot fail to confuse, if not mislead, the student, since in the two phyla (Mollusca and Annelida) next described this is not the case.

In matters of theory there will always be divergence of opinion, and we cannot help thinking that the student would have gained a notion of the nature of the coelom far clearer and nearer the truth, if Mr Sedgwick, in this volume at all events, had merely, without theoretical bias, stated the plain matter of fact: that from the Platyhelminths upwards we find a cavity in the mesoblast into which the genital cells are shed, and from which they are led to the exterior by special ducts, that in the lower forms this cavity is small, whilst in the higher it becomes large, and forms what we call the coelom.

In the succeeding chapters, the relations of the coelom to the haemocoele, and its division in the Mollusca into pericardial, renal, and genital regions, are all explained with admirable clearness.

As usual in text-books, the terminology of the excretory organs is somewhat confused. The term nephridium is not applied to the excretory ducts of the Platyhelminths, but to the renal coelom of the Mollusca, and the segmental excretory organs of the earthworm. We are further told that "the renal organs, called nephridia, are part of the coelom," *i.e.*, according to the author's theory, of the enteron. Now, is there a particle of evidence to show that the nephridia of the earthworm have ever formed a part of the enteron, or even of the coelom?

In the excellent chapter on the Annelida two things may be noticed. Discussing the homology of the prostomium, Mr Sedgwick argues that it probably corresponds to a real segment, since mesoblastic somites are found in front of the mouth in *Peripatus*. Surely we should not argue from the Arthropod to the Annelid, but from the Annelid to the Arthropod. The evidence, when taken in its proper order, leads us to an entirely opposite conclusion. It is strange, also,

to meet with the statement on page 467 that there are no special genital ducts in the Polychaeta. Such organs have long been known to exist in the Capitellidae, and are indeed mentioned on page 486 by the author himself.

Mr Sedgwick very wisely rejects the group Gephyrea, placing the Echiuridae with the Annelida, and the Sipunculidae and Priapulidae by themselves. On the other hand the Entoprocta are still retained in the Phylum Polyzoa with the Ectoprocta. This treatment inevitably leads to inconsistencies such as the following: the Polyzoa are defined as animals "with coelom"; turning to the definition of the Entoproctous group, we find it stated that "there is no body-cavity."

Good accounts are given of *Phoronis*, *Sagitta*, and the Brachiopoda, together with very interesting discussions of their affinities. The one on the Brachiopoda seems to us a model of what such a discussion should be in a text-book of this kind—clear and definite without being biassed, interesting and suggestive without being controversial.

We have perhaps already indulged too much in adverse criticism; it is so easy to find fault. There is, however, just one more point to be mentioned. Mr Sedgwick considers that the resemblance between *Phoronis* and the Polyzoa Phylactolaemata is not real, that the line between the mouth and the anus in the first case is dorsal, and in the second ventral. We must confess that the arguments brought forward to support this view seem to us quite inadequate, and we believe few zoologists will agree with the author in this matter.

The volume is well got up, excellently printed, and illustrated by a large number—nearly 500—of well executed, clear, useful figures. The majority of these are quite familiar, being chiefly drawn from Claus' 'Lehrbuch'; but there are some 50 new illustrations, generally well selected, except fig. 398 of *Arenicola*, which is both ugly and inaccurate.

We hope the second volume will soon be published. If it is as good as the first, this work, when completed, will form one of the most useful and reliable Text-books of Zoology yet written.

E. S. G.

For Museum Curators

Museums Association: Report of Proceedings, with the Papers read at the Eighth Annual Meeting held in Oxford, July 6 to 9, 1897. Edited by James Paton. 8vo, 124 pp. London: Dulau & Co., 1897 [?!]. Price, 5s.

The Superintendent of Corporation Museums and Art Galleries at Glasgow succeeds the secretaries as editor of this publication, and succeeds them also in their curious habit of publishing on the very title-page a statement hardly consistent with strict accuracy. We do not know how the most casuistical of Scotchmen could reconcile the imprint "London, 1897," with the review of various publications that certainly were not issued before 1898. Our copy was received on April 22.

Last year the Association visited Oxford, and the president was Prof. Ray Lankester. His address is chiefly remarkable for a much needed attack on "that enemy of the human race, the eminent architect . . . who deliberately and habitually perverts the funds entrusted to his discretion, so as to produce a showy and expensive building, whilst ignorantly and shamelessly neglecting the essential purpose for which the building is required." Following up this idea,

the President suggests that a committee should be appointed to report upon two questions: (1) "What is the best form and arrangement of rooms in a building intended to serve as a museum? (2) What is the best way of exhibiting specimens, according to their various kinds, in a museum?" The rest of the address is taken up with an account of the state of museums at Oxford, and of Prof. Lankester's own attempt to follow the principles of Flower and Brown Goode in the face of considerable difficulties.

Entomologists will welcome Prof. Poulton's account of the methods of setting and labelling Lepidoptera for museums. Prof. Miers' description of the arrangement of the mineral collection in the University Museum will also be of much service to those in charge of similar collections, although the philosophical scheme adopted by him is, as Mr Rudler said, hardly suitable for collections intended for the use of miners and such practical folk. A paper of somewhat novel character is that by Mr Harlan J. Smith, of the American Museum of Natural History, on Popular Museum Exhibits. Everyone knows that there are a number of people of various ages who use museums as promenades, doss-houses, or playgrounds. If the attention of some of these can be arrested by a sensational exhibit they may perhaps be led to look at something more, and thus the museum may be brought into contact with an entirely fresh class.

The recognition of museums by the Council of Education has set fresh problems before the museum curator, and gives rise to an interesting discussion opened by Mrs Tubbs, former member of the Hastings School Board. Another discussion of much importance was that of Dr Flinders Petrie's scheme of a federal staff for museums, that is to say, a band of peripatetic specialists who should visit museum after museum, naming specimens, lecturing, and supervising the exhibits in his own particular department. Mr Goodrich's valuable notes on museum preparations have already found publication in our pages.

The report of the meeting is followed by various "General Notes" by the secretary, Mr E. Howarth. Among these are short abstracts based on "reports and handbooks received from various museums belonging to the Association." We should have thought that it would have been of more value to members of the Association to have given them an account of museums that had not yet entered the select circle; but Mr Howarth, we are glad to see, considers that full information upon these matters can always be obtained from *Natural Science*, "an indispensable monthly journal in all museums," so that we are not inclined to insist upon our criticism. We are glad, too, to notice that the Association has now opened its membership to museums outside the limits of the United Kingdom, and that advantage of this has been taken by the museums of Baroda, Colombo, Jamaica, Western Australia, Salt Lake City, and the Australian Museum. We are, however, somewhat surprised to see how comparatively few museums of the United Kingdom have availed themselves of the advantages offered by this Association. Does this point to indifference on the part of their curators, or to some fault of which we are not aware in the working of the Association? Or is it merely due to want of advertisement?

Botany from the German

A Text-Book of Botany. By Drs E. Strasburger, F. Noll, H. Schenck and A. F. W. Schimper. Translated from the German by H. C. Porter, Ph.D. 8vo, pp. x+632, with 594 illustrations, in part coloured. London: Macmillan & Co., 1898. Price, 18s. net.

Lehrbuch der Botanik für Hochschulen. Von Drs E. Strasburger, F. Noll, H. Schenck und A. F. W. Schimper. Dritte verbesserte Auflage. 8vo, pp. viii+570, with 617 illustrations, in part coloured. Jena: Fischer, 1898. Price, 7 M. 50 Pf. in paper covers; 8 M. 50 Pf. bound.

When we reviewed the original German edition of this book shortly after its appearance in 1894 (*Natural Science*, vol. vi., p. 423) we congratulated the publisher (Fischer of Jena) on the issue of a text-book of the first order for the small sum of seven shillings, and at the same time hinted that a translation into English had been arranged. The translator, Dr Porter, assistant instructor in Botany at Pennsylvania University, has taken more than three years over his work, and in the meantime the 'Lehrbuch,' which has apparently met with a well-deserved success, has run into a third, much improved, edition. So the book as it is put into the hands of our students is two editions behind, which, considering certain advances in knowledge and modifications of views which were accepted four years ago, places its readers at some disadvantage. Apart from this defect the edition now before us is a good reproduction of the original text, for which we believe some thanks are due to Mr A. C. Seward, who revised the proofs. It is, however, unlikely that the translation will meet with the success that has attended the 'Lehrbuch.' This might have been the case had the publishers seen their way to issuing it at half-a-guinea, but at its present price it is the most expensive text-book of its kind, and has, moreover, to compete with one which, though in some respects inferior, is three shillings cheaper, and has already become established in many of the English botany schools. It is matter of special regret that only a small number of students should have the advantage of reading Prof. Strasburger's excellent introduction to Morphology, which is by far the best of the four sections. Dr Noll's contribution on Physiology is unequal, though fair on the whole, but the section dealing with the Cryptogams, by Dr Schenck, is perhaps the least satisfactory, the chapters on the Bryophyta and Pteridophyta are scarcely full enough. Prof. Schimper's account of the families of seed-plants is useful and well-arranged.

The foregoing criticisms as to the relative value of the different sections of the book apply also to the third edition of the 'Lehrbuch.' Prof. Strasburger remains *facile princeps* with his introduction to Morphology and Anatomy. In this part also we find the most evidence of revision and bringing up to date. The intimate structure of the cell, its nucleus, and other contents is at present one of the most favourite objects of study, and the modification of former views or the birth of new ones find expression in the text-book in the alteration of old figures and the introduction of new. We regret to find in the section on Physiology that the antiquated explanation of the formation of starch as the first product of assimilation by direct union of carbon dioxide and water still finds a place. The equation is an absurd one from the purely chemical as well as from the physiological point of view.

The small increase in size of this third edition (the original con-

tained 558 pages and 577 plates) is in part accounted for by the introduction of a bibliography at the end of the book. This is a useful addition, and one which saves space in the text, as references are now given merely to a number to which the corresponding citation can be found in the list. Part of the increase in bulk is due to the extra figures, forty in number, a large proportion of which are coloured, including some officinal as well as poisonous plants. The new coloured blocks are better than the old, but though they certainly enliven the pages and often give a better idea of the plant than the average black and white figure, we question whether the advantages gained are sufficient to justify the increased cost of production. Certainly this is not the case in the English edition, where the colour-printing shows to far less advantage.

The Birds of India

The Fauna of British India, including Ceylon and Burma. Edited by W. T. Blanford. Birds, Vol. IV., by W. T. Blanford, F.R.S. 8vo, pp. i-xxi+1-500. London: Taylor & Francis. Price, 15s.

It is with unfeigned satisfaction that we hail the completion of this important series, which is calculated to further in no unimportant degree our knowledge of the zoology of the Indian Empire. When noticing the appearance of the third volume of the "Birds" (*Natural Science*, VIII., p. 46, Jan. 1896), we deprecated any undue haste in the completion of the fourth volume, which was then under weigh. Little more than two years have since elapsed, and yet Mr Blanford has contrived to deal exhaustively with no fewer than twelve important Orders, a task entailing enormous labour both in the museum and the library. No doubt the undertaking has been considerably facilitated by the volumes of the British Museum Catalogue which have recently been drawn up by Messrs Ogilvie-Grant, Howard Saunders, Dr Sharpe, and Count Salvadori; but Mr Blanford has expended an immense amount of thought and trouble upon his new book. We notice, by the way, that he is unable to recognise the validity of *Merganser comatus* of Salvadori. This Himalayan form of the Goosander (*M. castor*) is usually just recognisable by its slightly shorter bill, and rather narrower black borders of the tertiary quills in the male; but we think that Mr Blanford has acted wisely in ignoring these fine distinctions. Mr Finn's re-discovery of the Eastern White-eyed Pochard as an Indian bird shows how much good work remains to be accomplished by up-to-date workers in that country. A good figure of *Nyroca baeri* may, of course, be consulted, by referring to the work of its original discoverer, Radde ("Reisen im Süden von Ost-Siberien," Taf. xv.). We question whether Mr Blanford was justified in including *Anser brachyrynchus* in the Indian 'Ornis,' in the absence of any authenticated specimens. It is at least as likely that the Pink-footed Geese recorded by Blyth, Irby, and others may have belonged to the recently described *Anser neglectus* of P. Sushkin (*cf. Ibis.* 1897, pp. 5-8). H. A. M.

A New Ornithological Serial

Avicula, Giornale Ornitologico Italiano per lo studio dell' Avifauna Italica, e per tutto quanto ha relazione con gli uccelli in generale. Direttore Cav. S. Brogi, Siena. Nos. 1-8, 1897, 1898. Annual subscr., 4 francs 50, post-free.

The pursuit of ornithology finds so many enthusiastic devotees on the Continent, that the establishment of an organ intended to deal speci-

ally with the birds of the Kingdom of Italy was a particularly happy thought. It must be gratifying to Cav. Brogi to find himself so well backed up by his countrymen. The eight numbers of *Avicula* which have now made their appearance contain many fugitive notes upon rare birds, such, for example, as *Chetusia gregaria* and *Charadrius fulvus*, which rarely journey sufficiently far west to offer sport to Roman wildfowlers. But the mainstay of *Avicula* is to be sought for in the admirable series of papers contributed by able observers upon the birds of their own districts. Sacerdote Antonio Tait is penning a noteworthy report upon the Avifauna of Trent; while Armando Lucifero has recently commenced a dissertation upon the ornithology of Calabria, a region which has hitherto received less attention from naturalists than the provinces of Northern Italy. We wish continued prosperity to this plucky venture of our colleagues.

New Serials

We have received the lengthy announcement of a forthcoming periodical, entitled *La Industria Agricola*, which is to be published at Caracas in Venezuela. Each number will consist of 32 octavo pages, and will appear monthly.

We learn from *Science* that a handsomely illustrated periodical, entitled *Monumental Records*, and edited by the Rev. H. M. Baum, is published at Box 1839, New York City, at a price of $1.50 per annum. It is concerned with the discovery of ancient monuments in both the Old and New Worlds.

We learn from the *Scottish Geographical Magazine* that the Geographical Society of La Paz, Bolivia, founded in 1889, has begun the publication of a journal. The first number contains a monograph of the Province of Muñecas, an account of the Province of the Mojos, written two centuries ago by a Jesuit missionary, and a statement of the boundary dispute with Peru.

The Oxford University Junior Scientific Club has recently exchanged the publication of a more or less contemporary *Journal* for *Transactions*, appearing at rarer intervals, and containing only the papers read. The first two parts were published in 1897, a more exact date not being given. The first contains a criticism by Mr Garstang of Dr Gaskell's Theory of Vertebrate Ancestry, with a restatement of Mr Garstang's own views as to the Echinoderm relations of the vertebrate ancestor. The next paper is by Mr E. C. Atkinson, and it is an abstract of one which we shall publish in our next number. This is followed by an abstract of A. E. Boycott's paper, "Shell Coloration (he calls it 'colouration') in British extra-marine Mollusca." No. 2 contains some stray notes on the birds of Oxford by A. W. S. Fisher. His paper is the only one honoured with a report of the discussion. Nos. 3 and 4 were issued together in May 1898, and contain the abstract of a paper on Athletic Training, a subject always popular at Oxford, by G. W. S. Farmer. Although outside our scope, we can hardly pass without notice the excellent and enthusiastic account of the life and work of the illustrious chemist, Victor Meyer, by F. Soddy.

Bibliographical

The Catalogue of the United States Public Documents, which is issued monthly by the Superintendent of Documents at the Government Printing Office, Washington, gives in its January issue (No. 37) a partial list of United States Government publications on Alaska. This should be of value to many besides those who are making their way to Klondike.

The *Transactions of the Geological Society of South Africa*, vol. iii., in its complete form, is just to hand, bearing date 1st February 1898. It is made up, as usual, of various parts, which were issued separately at the dates borne upon their several title-pages. The volume contains a great deal of important information on the geology of South Africa, and gives a map of the Klerksdorp goldfields, to accompany Mr Bawden's paper thereon. We note from the President's address that the Society has been so fortunate as to secure Prof. Rupert Jones' unique and valuable collection of books, pamphlets, maps, etc., on the geology of the Colony, and thus is enabled to found a library under conditions which few colonies have enjoyed. A commendable addition to the present volumes is the "Complete Index to all Papers published from 1st February 1895 to 1st February 1898," and we hope that librarians will not tear it up and throw it away because it is part of a wrapper. The wrapper has often proved to be the most essential part of a serial publication, and should never be destroyed, but left absolutely intact and in place as issued, when the volume is bound.

We have received from Messrs Dulau & Co. sections 50-52 of their "Catalogue of Zoological and Palaeontological works." These sections consist of 48 pages, dealing with the literature of Diptera, Hemiptera, and Hymenoptera.

Further Literature Received

Catalogue of Welwitsch Collection, Part II., Hiern: British Museum. Pruning-book Bailey; Text-book of Entomology, Packard: Macmillan Co., N.Y. Die Zelle und die Gewebe, O. Hertwig; Grundriss d. vergleichende Anatomie d. Wirbelthiere, Wiedersheim: Fischer, Jena. Practical Plant-Physiology, Detmer transl. Moor; The Wonderful Century, Wallace: Sonnenschein. Types of Scenery, Romanes Lecture, A. Geikie: Macmillan, London. Medical Missions, Acland: Frowde, London. Text-book of Zoology, Wells & Davis: Univ. Tutorial Series, Clive, London.

A propos de l'Eglise et de la Science, Erréra. Ratzel's History of Mankind, Part 26. Cotgreave's Subject-Index, 11-14. Le protoplasma radio-conducteur, Morin: *L'opinion médicale*. La pisciculture pratique, Baudouin: *Bull. Soc. Pêche France*. Jamaica Fisheries, Duerden: *Daily Gleaner*. Root-tubercules, Otis: *Industrialist*. Obsidian "buttons" in Tasmania, Twelvetrees & Petterd: *Proc. R. Soc. Tasmania*. The Cubo-Medusae, Conant: *Mem. Biol. lab. Johns Hopkins*. Birds from St Marta, Bangs; Squirrels from Mexico, Nelson: *Proc. Biol. Soc. Washington*. Simple Ascidians from Puget Sound, Herdman: *Trans. L'pool Biol. Soc.* Crustacea of Norway, vol. ii., parts 9, 10, G. O. Sars; Bergen Museum. Catal. de la Bibliothèque, Fasc. xxiv.: *Feuille jeunes Nats.*

Scot. Geogr. Mag., June; Amer. Journ. Sci., June; Amer. Nat., April; Victorian Nat., April, May; L'Anthropologie, March and April; Botan. Gazette, May; Feuille des jeunes Nat., June; Irish Nat., June; Westminster Rev., June; Knowledge, June; Literary Digest, May 21, 28, June 4; Naturae Novit., March, Nos. 5, 6, April, Nos. 7, 8, May, Nos. 9, 10; Naturalist, June; Nature, May 19, 26, June 2, 16, and Supplement; Nature Notes, June; Naturen, May; New Age, April; Oxford Univ. Jr. Sci. Club Trans., May 4, 1896, No. 37, June 1897, N.S. 1897-98, Nos. 1, 2, 3, 4; Photogram, June; Plant World, May, June; Psychol. Rev., Monogr. Supplement, May, June; Review of Reviews, May, June; Revue Scient., May 21, 28, June 4, 11; Science, May 13, 20, 27, June 3; Riv. Psichologia, May 15, 31; Scientific Amer., May 14, 21, 28, June 4; Scot. Med. and Surg. Journ., June.

OBITUARIES

CHARLES HERBERT HURST

BORN SEPTEMBER 1855. DIED MAY 1898

DR C. H. HURST was an alumnus of the Manchester Grammar School, where he gave an indication of his future bent by attaining a high position on the science side. After an interval spent in business he entered the Royal College of Science under Professor Huxley, and in 1881 was bracketed equal with his friend Dr John Beard at the head of the list in biology. In the same year he entered the Owens College as a student, and in 1883 was appointed demonstrator and assistant lecturer in this institution under the late Prof. Milnes Marshall.

He filled this position for more than eleven years, and earned the gratitude of the students by his clear and vigorous teaching and by his constant readiness to assist all who were in earnest in their studies. In 1895 he left the Owens College, much to the regret of his colleagues, in order to fill a similar post in the Royal College of Science, Dublin.

Hurst's published writings are not numerous. Ill health prevented him from doing much more than the engrossing nature of his college duties demanded. His most important work was probably his share in the production of the "Junior Course of Practical Zoology," which has made the names of "Marshall and Hurst" household words wherever biology is studied. Many of the drawings are from his pencil, for he was an excellent draughtsman; and almost the whole work was done by both authors, very few paragraphs being written by either alone.

In 1889 he took advantage of a prolonged leave of absence to study in Leipsic under Leuckart, where he made an interesting addition to our knowledge of the developmental history of *Culex*, for which he was awarded the degree of Ph.D. In 1891 he undertook a new line of zoological work, in the shape of a systematic criticism of Biological Theory. The result of this was a series of papers published in *Natural Science*, in which many popular and orthodox views were attacked in a trenchant and unsparing manner, which, though it could cause no offence to those who knew the man and his honesty of purpose, was undoubtedly misunderstood by some who were not personally acquainted with him. Space forbids entering in detail into these papers individually; it must suffice to mention as examples "The Nature of Heredity" (*Nat. Sci.*, vol. i.), "The Function of Tentaculocysts" (*Nat. Sci.*, vol. ii.), "The Recapitulation Theory," and the series of papers on *Archaeopteryx*, and to say that they contain many observations of force and justice, though some of

the conclusions are by no means accepted by those who have worked over the same ground. Dr Hurst was a man of wide reading, a keen controversialist, and a staunch friend of those few who knew him intimately. W. E. H.

OSBERT SALVIN

BORN 1835. DIED 1ST JUNE 1898

WE deeply regret to record the death of this amiable and kindly man, this accomplished ornithologist and entomologist, who has done so much for zoological science. Mr Salvin passed away peacefully in his sleep. He had known for years that his heart was in so weak a state that life was extremely uncertain; but this in no way discouraged or dismayed him.

Mr Salvin was the second son of the architect Anthony Salvin, and was born in 1835. He was a Westminster boy, passing to Trinity Hall, Cambridge, graduating in 1857. On taking his degree he went to Tunis and Eastern Algeria on a natural history expedition in company with Mr W. H. Simpson (afterwards Hudleston) and Mr (now Canon) Tristram. They were away five months, and on the return of the party, in the autumn of the same year, Salvin proceeded to Guatemala where, chiefly in company with the late G. U. Skinner, the orchid hunter, he stayed until the middle of 1858, returning again to Central America about twelve months later. In 1861, Salvin again went to Central America, accompanied by F. Du Cane Godman, to continue his explorations, but returned home in 1863. Marrying in 1865, he, with his wife, undertook a fourth trip to Central America. In 1874 he accepted the first Strickland curatorship in the university of Cambridge, and filled the office until 1883, when on the death of his father, he succeeded to Hawksfold, near Haslemere. About this time he threw all his energies into the "Biologia Centrali-Americana," a work conceived by his friend and colleague Du Cane Godman and himself, and now having reached many volumes. This "Biologia" was schemed to be a complete natural history of all the countries between Mexico and the Isthmus of Panama, and is remarkable not only for the wealth of material it places at the disposal of the zoologists, but for the regularity of issue, and the care with which details of publication and other matters are considered. Salvin edited the third series of *The Ibis*, of which he was one of the founders, brought out a "Catalogue of the Strickland Collection" in the Cambridge Museum, and contributed the sections on the Trochilidae and the Procellariidae to the British Museum Catalogue of Birds, while his latest work was the completion of the late Lord Lilford's "Coloured Figures of British Birds." He was the author of some 150 papers in scientific publications, many jointly with Mr Godman, or with Mr Sclater.

Mr Osbert Salvin was one of the most kindly, amiable, and unassuming of men, who had endeared himself to a large circle, not the least part of which was the younger generation; and his loss, though severe enough to zoological science generally, will be still more keenly felt by those whom he had encouraged and befriended.

We are indebted to a sympathetic notice in *The Times* for much of the above information.

EDWARD WILSON

Born 1848. Died May 21, 1898

British geologists are mourning the death of one of their most respected and energetic colleagues, Mr Edward Wilson of Bristol. Born at Mansfield, in Nottinghamshire, fifty years ago, his attention was early directed to geological subjects, and when only fifteen years of age he wrote an essay on "The Coalfields of Derbyshire," which won for him a special prize at the Nottingham High School. For fourteen years he was a teacher of the science classes in the Nottingham Mechanics' Institute, and during this period he devoted all his leisure to the study of the geology of south Derbyshire and Nottinghamshire. He published several important papers embodying his results, and in 1881 he received the Darwin Medal of the Midland Union of Natural History Societies in recognition of the value of his work. Among more general subjects, Mr Wilson's discussion of the age of the Pennine Chain in the *Geological Magazine* will be specially remembered. In 1884 he was appointed Curator of the Bristol Museum, in succession to the late Mr E. B. Tawney, and amid the trying vicissitudes of that institution he continued to fulfil the duties of the curatorship with enthusiasm until the time of his premature death. While in Nottinghamshire, Mr Wilson had paid special attention to the Triassic and Rhaetic formations, and when removed to Bristol he was able to extend his researches to the same strata in a new field. One of his most important stratigraphical papers, indeed, referred to the Rhaetic rocks of Pylle Hill, Bristol (*Quart. Journ. Geol. Soc.*, 1891). This was followed in 1896 by a still more valuable paper on the Lower Oolites of Dundry Hill, written in co-operation with Mr S. S. Buckman. Facilities at Bristol also enabled Mr Wilson to devote much attention to Palaeontology, and he studied with success the Gasteropoda of the British Jurassic formations. With Mr Hudleston he published a valuable Catalogue of the British Jurassic Gasteropoda in 1892; and at the time of his death he was occupied with a memoir on the Gasteropoda of the Lias for the Palaeontographical Society. In 1888 the Geological Society of London awarded to Mr Wilson the balance of the Murchison Fund in token of appreciation of his researches. He was an unobtrusive worker whom to know was to admire; and his untimely death leaves a sad gap in the ranks of those geologists who combine painstaking field-work with still more laborious study in the museum.

MAURICE JEAN ALEXANDRE HOVELACQUE

Born 1858. Died at Passy, May 17, 1898

Dr Hovelacque was a good representative of a School of French botanists; he worked for some time under Prof. Bertrand of Lille, and his researches have been chiefly conducted on the lines adopted by Bertrand and other French anatomists. He contributed several papers on the minute structure of the vegetative organs of flowering plants, and eventually published a comprehensive treatise on the anatomy of certain families of Dicotyledons.[1] This work is the result

[1] "Recherches sur l'appareil végétatif des Bignoniacées, Rhinanthacées, Orobanchées et Utriculariées." 8vo, 765 pp. Paris, 1888.

of much careful and detailed investigation in a branch of plant anatomy which has received considerable attention in recent years at the hands of French and German botanists. Dr Hovelacque's death has deprived palaeobotanical science of a keen and able worker; his monograph on *Lepidodendron selaginoides*, published in the *Mémoires* of the Linnean Society of Normandy in 1892, is a work of considerable merit, and is recognised as one of the best recent memoirs on the anatomy of *Lepidodendron*. In recent years municipal duties occupied much of Hovelacque's time; and he always regretted that circumstances prevented him devoting himself entirely to scientific research. He was in strong sympathy with English science, and with his confrères on this side the Channel.

We regret also to record the following deaths:—AUGUST ASSMANN of Breslau, student of Lepidoptera; on April 25, at Berkeley, Cal., U.S.A., aged 86, MELVILLE ATWOOD, F.G.S., noted for his metallurgical discoveries in the first half of the century; on February 10, at Oudenbosch, Holland, VICTOR BECKER, the anthropologist; J. GALLOIS, entomologist and anthropologist at Déville lès Rouen; on April 29, aged 82, SAMUEL GORDON, M.D., President of the Royal Zoological Society of Ireland from 1893 till the end of 1897; the Rev. WALTER GREGOR, a zoologist, at Pitsligo, Aberdeen; on March 30, aged 53, JAMES I'ANSON, mineralogist and director of the Technical College at Darlington; JOSEF JEMILLER, a student of Hymenoptera at Munich; Dr S. KELLICOTT, Professor of Zoology at Ohio State University, and general secretary of the American Association for the Advancement of Science; on April 12, at Meran, aged 49, FERDINAND KRAUSS, geographer and anthropologist; on April 5, at Gross Lichterfelde, near Berlin, Prof. LEOPOLD KRUG, noted for his studies on the West Indian flora, aged 63; on May 11th, aged 75, W. C. LUCY, F.G.S., sometime president of the Cotswold Naturalists' Field Club, to the proceedings of which he contributed several papers on geological subjects; on April 10, in London, aged 82, General E. H. MAN, noted for his anthropological studies in the Andaman Islands; W. M. MASKELL, Registrar of the University of New Zealand, and well known for his researches on the Coccidae; on April 29, at Celle, Hanover, aged 83, Dr K. NÖLDEKE, palaeontologist; on April 7, at Arcachon, aged 37, MARTIAL JEAN MAURICE NOUALHIER, a student of the Hemiptera, specially interested in the fauna of the Canary Islands; MARIANO DE LA PAZ GRAELLS, professor of Comparative Anatomy at Madrid University, on February 13, at Madrid, aged 80; at Yalta, Crimea, LEONID PAVLOVITSCH SSABANEJEV, zoologist and editor of the Russian *Nature and Sport*; on March 27, at St Petersburg, Dr GUSTAV SIEVERS, an entomologist and explorer of Upper Armenia and the Trans-Caspian provinces; on April 2, aged 64, the well-known histologist, SALOMON STRICKER, professor of Pathology at Vienna University; on May 5, the student of Diatoms, EUGEN WEISSFLOG, aged 75, at Dresden; on February 20, at Munich, aged 75, CONRAD WILL, the former director of the zoological collections there.

NEWS

The following appointments have been announced :—Mr H. C. Chadwick, to be curator of the biological station at Port Erin, Isle of Man ; Prof. John Vinezielt, to be director of the bacteriological laboratory and assistant professor of biology at the University of New Mexico at Albuquerque, having F. S. Maltby of Johns Hopkins University as assistant in the bacteriological laboratory, and E. G. Coghill of Brown University, as assistant in the biological laboratory ; Prof. G. Pruvot of Grenoble, to be chief of work in practical and applied zoology at the University of Paris ; Dr C. J. Cori, to be professor extraordinarius of zoology at the German University, Prague ; Dr B. Wandolleck of Berlin, to succeed L. W. Wiglesworth as assistant in the Zoological Museum at Dresden ; Dr Gertrude Haley, to be demonstrator in anatomy at Melbourne University ; Dr G. Ruge of Amsterdam, to be professor of anatomy at Zurich University ; Dr Ph. Stohr of Zurich, to be professor of anatomy at Würzburg ; Dr W. T. Porter, to be associate professor of physiology at Harvard.

Mr J. H. Holland, assistant curator of the Botanic Gardens, Old Calabar, in the Niger Coast Protectorate, to be curator in succession to Mr Billington ; Dr F. Noll of Bonn, to be professor of botany and director of botanical instruction at the Agricultural Academy of Poppelsdorf, Bonn, in place of Prof. Friedrich Körnicke resigned ; Dr Joh. Behrens, to be professor of botany at the Technical High School, Karlsruhe ; Dr Z. Kamerling of Berlin, to be assistant in the Botanical Institute of Munich University ; Paul A. Genty, to be director of the Botanical Gardens of Dijon ; Prof. L. Celakovsky, to be director of the newly opened botanical garden of the Bohemian University at Prague ; Mr Demoussy, to be assistant in plant physiology at the Natural History Museum, Paris ; Dr R. A. Harper of Lake Forest University, to be professor of botany at the University of Wisconsin ; Cornelius L. Shear of the University of Nebraska, to be assistant agrostologist in the U.S. Department of Agriculture.

Dr Max Blanckenhorn of Erlangen, to be temporarily on the staff of the Egyptian Geological Survey ; Prof. A. E. Törnebohm, to be director of the Geological Survey of Sweden in succession to Dr Otto M. Torell, resigned ; Mining Engineer N. Th. Pogrebov, to be secretary and librarian of the Geological Committee, St Petersburg ; A. Lawrski, to be privat-docent in mineralogy and geognosy at Kazan University ; N. Andrussow, to be associate-professor of geology and palaeontology at Dorpat (Jurjew) University ; Dr Heinrich Ries of Columbia University, to be instructor in economic geology in Cornell University.

Sir William Flower has received from the German Emperor the Royal Prussian order "Pour le Mérite" for science and art.

Dr John Murray of the 'Challenger' has been made K.C.B., and we beg to offer him our warm congratulations.

The Queen has nominated Mr Charles Sissmore Tomes, F.R.S., F.R.C.S., L.D.S., to be for five years a member of the General Council of Medical Education and Registration of the United Kingdom.

The proposal to establish a Final Honour School of Agricultural Science at Oxford University has been rejected by Congregation. We may contrast with

this the statement that the preparation for the extension of agricultural teaching under the auspices of the Cornell University has this year been increased from $25,000 to $35,000.

The Rolleston Prize of the Universities of Oxford and Cambridge has been awarded to Richard Evans, of Jesus College, Oxford, for his memoir on "The Development of Spongilla." The examiners consider the memoirs "On the Coagulation of the Proteids of White of Egg," sent in by W. Ramsden, of Pembroke College, Oxford, to be of great merit, and worthy of honourable mention.

Miss Cruickshank has presented the Aberdeen University with £5,000 for the formation of a botanic garden, to be named in memory of her late brother, Dr Alexander Cruickshank.

A Chair of Agricultural Zoology has just been created at the Faculty of Sciences of Marseilles, and Dr A. Vayssière has been appointed to the Chair. Publications will be issued from the laboratory, and foreign publications of similar nature are solicited in exchange.

The University of Chicago proposes to establish Doctorate Fellowships, or, as we should call them, Research Fellowships, with an annual income of $750. Candidates must have received the degree of Ph.D. from the University of Chicago, and their appointments must be approved by the officers of the department or departments in which their proposed research falls. They will be expected to work for nine months of each year at the University, and to prepare the results of their researches for publication. Appointments are to be made annually, but may be confirmed for a period not exceeding five years.

The Trustees of the British Museum have recently purchased the large collection of marine animals formed by Canon A. M. Norman, and containing type-specimens of many species which he has established. Part of the collection is already in the Museum, the rest will go there eventually.

The Bentham Trustees have recently presented a portrait in oils of Robert Brown to Kew Gardens.

The Edinburgh Museum of Science and Art has recently acquired the valuable collection of fossils from the Upper Silurian rocks of the Pentland Hills, made by the late David Hardie, of Bavelaw. It is specially rich in specimens from the Eurypterid beds of Gutterford Burn near Carlops, Peeblesshire ; there are also specimens, chiefly sponges, from North Esk.

In the Museum of the Scarborough Philosophical and Archaeological Society, Mr C. D. Head has been replacing the old and ruinous collection of birds by cases displaying them, so far as possible, in their natural habitat, along with their nests and eggs, when these can be obtained. An improvement has also been made in the cases for the fossils. The Society records the capture of two badgers—one at Cloughton, the other near Folkton. The record of local birds has been placed upon a more satisfactory basis, every item contained in the list being thoroughly authenticated. We are glad to see that the fish, both sea and fresh-water, also are being studied ; Mr F. Grant records the occurrence of various species not hitherto observed. Considerable attention is also paid by members of the Society to the Invertebrata of various Classes, though naturally the land and fresh-water Mollusca and the Lepidoptera come in for the giant's share. The geologists have paid attention to the exposures during the making of the Marine Drive, but not many fossils have yet been found. Other items of much interest to local naturalists are contained in the *Report* for 1897, which shows that the Society is in a more satisfactory condition scientifically than it is financially.

The Museum Committee of the Council of the City and County of Bristol have just been obliged, through the lamented death of Mr E. Wilson, to advertise for a curator. They want "a Gentleman," a scientific man "competent to catalogue the collections in and to advise as to the acquisition of specimens for the Museum;" he will have to act also "as Secretary to the Committee" and "to devote the whole of his time to the duties." In return for the absolute service of this highly educated gentleman, the Museum Committee of this wealthy city offers a salary of £200 per annum. It is about time for the British Association to meet in Bristol again, if only to arouse in the minds of these economical councillors more intelligent appreciation of modern science than their advertisement betokens.

The Nottingham Natural History Museum has recently acquired a collection of eggs and skins of British birds, formed by Mr F. B. Whitlock.

There are now exhibited in the zoological galleries of the Paris Museum of Natural History the collections made by Count de la Vaulx in Patagonia. Among these, says *L'Anthropologie*, are more than a hundred skulls or skeletons, and other ethnographic objects of great importance. During the months of April, May and June there has been delivered in these galleries a series of lectures on natural history and scientific observation for travellers, with practical demonstrations. The lecturers included many of the leading naturalists of Paris.

At the Musée Guimet, Paris, are now exhibited many archaeological and ethnographic objects, collected by Baron J. de Baye during his recent journey in Siberia.

The *Camarasaurus* from South Wyoming has now been mounted and set up in the American Museum by Prof. Osborn and his assistants. It is 62 feet long.

The Government Museum of the South African Republic at Pretoria, under the direction of Dr J. W. B. Gunning, favours us monthly with a list of its accessions. This appears to evidence a praiseworthy activity on the part of the Museum, but we note that the greater number of the specimens come from other parts of the world than South Africa, so that it does not throw very much light upon the collections that may be made in that country.

The meeting of the Museums' Association at Sheffield, under the presidency of Alderman Brittain, begins on Monday, July 4, with a visit to the Ruskin Museum. Papers and discussions will occupy the mornings, and visits to museums, steel-works, and water-works, the afternoons of the three following days. On Friday the members will visit Castleton. There is a strong Reception Committee, including the Lord Mayor, the Master Cutler, and the Duke of Norfolk. Papers will be read by Dr H. C. Sorby, Prof. W. C. F. Anderson, Prof. Denny, Mr S. Sinclair of the Australian Museum, Messrs F. A. Bather, H. Bolton, E. Howarth, and others.

The Conversazione of the Royal Society, held on May 11th, was from the point of view of natural science one of the most disappointing that we remember. There was a sentimental sort of interest in seeing the borings from Funafuti, but it was hardly to be expected that they should convey any information. Prof. Poulton's exhibit of insects captured in North America showed that he was an energetic collector who made good use of his holiday. But Prof. Poulton's energy was already well known to us. Models of vertebrae by Dr Gadow and Mr W. F. Blandford were of educational value, but chiefly striking for the gruesomeness of their colouring. The Marine Biological Association was represented by Mr Garstang, who showed crabs of different orders in their natural habitats in an attempt to prove that their ordinal characters are of an adaptive nature. This

thesis, however true, is not easily illustrated by such an exhibit. The perennial green oysters were brought up again by Profs. Herdman and Boyce, while even Dr Sorby's charming preparations of marine animals, as lantern slides, were somewhat lacking in novelty. A little more topical was the series illustrating the bacteriology of calf-vaccine lymph exhibited by Sir R. T. Thorne and Dr Copeman. Dr C. A. MacMunn is working on the digestive glands of Mollusca and Crustacea, and exhibited some microscopic sections of the same. Prof. Sherrington had some admirable microscopic preparations of the sensorial endings of nerve-fibres stained by gold chloride. Naturally the photographs of, and apparatus connected with, the recent solar eclipse cast everything else in the shade, while most people were attracted by Prof. Hele Shaw's experiments on the flow of water, and Prof. Oliver Lodge's magnetic space telegraphy. But the chief excitement of the evening was afforded by the efforts of various gentlemen, connected with the Science and Art Department, to induce eminent Fellows of the Royal Society to sign a petition to the Government with reference to the great South Kensington question.

Prof. Michael Foster is to be president of the British Association at its Dover meeting in 1899.

The Geological Society of London has elected as Foreign Correspondents, Marcellin Boule of Paris, W. H. Dall of Washington, and A. Karpinsky of St Petersburgh.

The Oxford University Junior Scientific Club held a successful conversazione at the University Museum on May 24. Lectures were delivered by Prof. H. B. Dixon on "Climbing in the Rocky Mountains"; by Dr Gustav Mann on "Microphotography"; and by Mr G. J. Burch on "Artificial Colour-Blindness."

The Preston Scientific Society, started some five years ago, appears to be doing useful work in propagating a knowledge of science in that neighbourhood. It numbers 520 members and has recently had granted to it by the Town Council of Preston the use of a Lecture Hall and other rooms in Cross Street. This Society does not confine itself to listening to lectures, but we read in the Annual Report that the members of the Botanical Section have studied the flora of the district within a radius of ten miles from Preston, with the object of forming a complete catalogue as well as a herbarium. The Geological Section proposes to study the geology of the neighbourhood. At Whitsuntide the Society organised an eight-day excursion to Oban; other summer excursions are to Owen's College, Trough of Bowland, Grange, Stonyhurst, Brock Bottoms, and Ingleton. The President is Dr Collinson; and the Secretary, W. H. Heathcote, 47 Frenchwood Street.

The Report of the Rugby School Natural History Society for 1897 shows a continuance of useful work, but there still appears to be a wide field of research open. Mr S. T. Dunn, who contributes some new botanical occurrences, writes: "Scarcely anything is known of the range and frequency of our water buttercups, brambles, roses, or pondweeds." The mosses and hepatics also require workers. The Entomological Section seems to confine itself, as is usually the case, to the study of Lepidoptera, while it is only natural that the report of the Zoological Section should deal chiefly with birds. It is always a difficult matter to attract the attention of schoolboys to the smaller and less popular plants and animals. The editors draw attention to the fact that one of the most important duties of a secretary of a section in such a society as this is to educate a successor; in this the secretaries who have recently left seem to have been quite successful.

The Report of the Cheltenham Natural History Society also shows that a good deal of active work is being carried on. We are glad to see that this Society, as well as that of Rugby, encourages phenological observations, and

publishes a list of the first flowerings for the year. In this Society the people of importance appear to be the presidents of the sections, who are all masters, but in the Rugby Society more is made of the secretaries of sections, who are, as they should be, boys in the school.

The S.E. Union of Scientific Societies met in Croydon on June 2, under the presidency of Prof. G. S. Boulger, who, in his address, contrasted the state of natural science sixty years ago with its present condition. A discussion on dene-holes was started by Mr C. Dawson, who regarded them as mines of chalk for agricultural purposes. Mr J. W. Tutt explained the difference between entomology as a scientific pursuit and the entomology of mere collectors. Mr C. Dawson read a paper, which will appear in our pages, on natural gas in Sussex. In our present number we publish the valuable and suggestive paper of Mr E. M. Holmes. Other papers were : by J. Logan Lobley on the place of geology in education ; H. Franklin Parsons on the soil in connection with the distribution of plants and animals ; E. Lovett on amulets and charms ; J. H. Baldock on photography in relation to science. More germane to the object of the Congress were the papers by J. M. Hobson and E. A. Pankhurst on ideals for Natural History Societies and how to attain them. Dr Rowe demonstrated the method of preparing fossils described by him in *Natural Science*.

The eighty-first annual meeting of the Société helvétique des sciences naturelles is to be held at Berne on August 1-3. T. Studer is president of the zoological and anthropological section ; A. Baltzer of the geological section ; E. Brückner of that of physical geography ; and Drs Strasser and Kronecker of anatomy and physiology.

Prof. Edward Suess, Vienna, Prof. H. de Lacaze Duthiers, Paris, and Prof. K. A. von Zittel, Munich, have been elected Foreign Associates of the American National Academy of Sciences.

The Grand Honorary Walker Prize of $1,000, awarded for five years by the Boston Society of Natural History, has just been awarded to S. H. Scudder, of Cambridge, Mass., for his work in entomology.

The coral-boring expedition to Funafuti will this summer resume work at the old bore at a depth of 698 feet. Lining pipes, which were on the former occasion lowered to a depth of 650 feet, will be reinserted and extended to the full depth. Boring can then be begun on the unproved rock, which is expected to be similar to that met with during the previous 30 feet of the old bore, namely, a white calcareous rock of about the consistency of hard chalk. Prof. David expects that the bed-rock will be reached within a depth of 200-300 feet from the bottom of the old bore. Early in August it is hoped that H.M.S. 'Porpoise' will bring from Samoa apparatus for putting down a bore in the bottom of the Funafuti lagoon. Commander F. C. D. Sturdee intends to moor his ship taut at low tide at a spot in the lagoon, which will be about a mile and a half westward from the main village. A boring platform will be fixed at the bows, whence pipes will be let down to the bottom of the lagoon, which at the spot selected is about 100 ft. deep. As soon as the pipes strike the bottom of the lagoon a powerful stream of water will be forced down them by means of a flexible hose connected with a large Worthington steam pump. It is hoped that then, if the bottom of the lagoon consists, as is thought probable, of soft and loose sedimentary material, a fair depth may be attained in the few days available for the use of the warship for this purpose. Work will be carried on at the lagoon day and night. It should be possible from time to time, by shutting off the water jet, and lowering a sand pump inside the pipes, to obtain small samples of the formation which is being penetrated. If this bore in the

lagoon is successful, it will much enhance the value of the main bore put down with the diamond drill.

The reason why it is proposed that the bore in the lagoon shall be situated only a mile and a half from the shore, instead of near the centre, is that one of the chief difficulties will be the danger of the ship dragging at her moorings. This would be intensified near the centre of the lagoon, where the full force of the squalls, trade winds, and strong currents would be experienced. At the spot contemplated, however, the warship should be not only out of the main current, but also somewhat sheltered on the coast by the thick belt of cocoanut palms and other trees with which the main island is densely wooded. After finishing the boring experiment in the lagoon, the 'Porpoise' will proceed to the Gilbert Islands, and on her return, early in September, she will be ready to pick up the diamond drill party, and convey them to Suva. Should, however, the main diamond drill bore not have been bottomed up to the date of the return of the warship to Funafuti, arrangements have been made by the London Missionary Society which will admit of their steamer the 'John Williams,' due at Funafuti in November, carrying the party either to Suva or New Guinea, whence they would return to Sydney. Our information is gained from an article in the *Daily Telegraph* (Sydney) for April 27.

At the anniversary meeting of the Royal Geographical Society it was announced that the Government had replied in a sympathetic manner to the appeal for an Antarctic expedition. Meanwhile, as Mr Borchgrevink's ship, the 'Southern Cross,' which leaves London in July, will fly the British flag, England will not be left wholly in the lurch by the numerous expeditions now being made to Antarctic regions.

To the numerous expeditions in the Arctic regions this year must be added a German one under the leadership of Mr Theodor Lerner, who is accompanied by Drs Brühl, Römer, and Schaudien. They started for the North Pole at the end of May on the ss. 'Helgoland.' Mr Walter Wellman, who is attacking the Pole from Franz Josef Land, is accompanied by Prof. J. H. Gore of Columbia University, who will make gravity determinations in Franz Josef Land ; Lieut. E. B. Baldwin, of the U.S. Weather Bureau ; Dr E. Hofma, of the University of Michigan as naturalists and medical officer ; and Mr Q. Harlan, of the U.S. Coast and Geodetic Survey.

The French Ministry of Public Instruction has sent Surgeon-Major Huguet to M'zab in Algeria, to continue his researches on the history of the country, and on the characters, commerce, industry, and medical customs of its inhabitants.

Prof. Robert Koch returned to Berlin on Thursday after an absence of a year and a half, spent in foreign travel for purposes of scientific research.

The Committee for the Huxley Memorial has issued a third donation list amounting to £1,058, 14s. 5d., the total amount now received being £3,346, 4s. 2d. Mr Onslow Ford is now engaged upon the statue, which is to be a seated one in marble, 8 feet high, and is to be placed in the central hall of the Natural History Museum. Dies for the Royal College of Science medal have been completed after the excellent design of Mr F. Bowcher. Copies in silver or bronze of the obverse of this medal bearing a profile portrait of Huxley, as well as replicas of the original model, can be purchased by subscribers to the memorial fund. Specimens of these are on exhibition in the architectural court of the South Kensington Museum till the end of September. After paying for the statue, the medal, and other expenses, about £1,300 remains, and, to quote the elegant phrasing of the Committee, "The nature of the contemplated third form of Memorial must largely depend upon the amount which may yet be subscribed."

CORRESPONDENCE

CORRIGENDA

May I ask those who have copies of my paper "Pentacrinus: a name and its history," either in vol. xii. of *Natural Science* or in its separate form, to correct the following misprints?

On p. 248, throughout the second paragraph, for C and D read B and C respectively. Through this misprint I appear to maintain the very proposition that the paper was written to disprove. I am indebted to the Rev. G. F. Whidborne for its discovery.

On page 253, second line from bottom, for 1897 read 1877.

7th June 1898. F. A. Bather.

THE STUDY OF VARIATIONS

I do not wish to trouble your readers with a prolonged and unedifying controversy, but I would only say that there is no question of rival theories, as Mr Tayler states, for when one sees plants rapidly changing their forms under one's very eyes, it is no more a theory to describe the process than to say trees put on their leaves in spring.

Mr Tayler's article is headed as above. Had Mr Baker of Kew written an article upon this subject, one would expect a treatise on roses or some such variable genus, for a scientific man would at first procure his variations in nature, and then study them. Mr Tayler reverses this process. He proposes a new system of classification, and when asked for examples, makes the astounding reply:—"I believe none are to be found in the present state of our knowledge on this question"! After such a confession it would be quite unprofitable to discuss his *d priori* assumptions, which he thus confesses have no bases in facts. George Henslow.

FOREIGN MONEY ORDERS

In the business relations of the Concilium Bibliographicum with its English subscribers we have had frequent occasion to note a very widespread misapprehension in regard to the foreign money order service which must seriously hamper those desirous of purchasing books or specimens abroad. It is really astounding how many letters we have received with the query, "But how can we remit such small sums?" To such we have replied by a simple statement of the procedure necessary, and, judging from the surprise manifested, I fancy such a statement may interest many of your readers. All that is necessary is to fill out an international money order-blank, to be had at any money order post-office, at a commission of 6d. for sums under £2. Such blanks call for the name and address of the sender and of the person to whom the money is to be paid, the amount, and the date. No further steps are necessary, the money being delivered by the letter carriers, who at the same time leave a statement showing the sum sent, the date, and the name and address of the sender. The issuing office gives a receipt to the sender showing to whom the money is payable. Thus, without even writing to the person to whom the money is to be paid, a foreign account may be paid and a receipt for payment received. Herbert Haviland Field.

[We must believe Dr Field's statement as to the ignorance of the British scientific public; but we agree with him that it is astounding.—Ed. *Nat. Sci.*]

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 29 and 30 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

To the Trade.—Natural Science is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

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SEP 23 1898

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 78—Vol. XIII—AUGUST 1898

NOTES AND COMMENTS

Sir William Flower

In consequence of failing health, Sir William Flower has found it necessary to resign his position as Director of the Natural History Branch of the British Museum. Both the cause of the resignation and the resignation itself will be deeply regretted by all naturalists in this country and abroad. In the charming volume of Essays, reviewed on another page of this number, Sir William tells us how he has been, from childhood upwards, a museum man. With the great expansion that followed on the removal of the natural history collections from Bloomsbury to South Kensington, Sir William found his opportunity, and he has availed himself of it to the full. To his staff he has been an inspiration and an example. Not one, not even the youngest, of his energetic and enthusiastic helpers can be said to have shown a more open mind, more desirous of proving all things and holding fast to that which was best for the display and arrangement of those wonderful collections. But there are other ways in which the head of an important institution of the State can make his influence felt, and Sir William's zeal for his Museum was never at rest. To urge the claims of the establishment on an unwilling Treasury, or (yet harder task) to extract sympathy from the ranks of power, wealth, and fashion, these formed the employment of what might have been his leisure hours. The high standing of our Natural History Museum, as well as the improvement in the character and position of its staff, are largely due to his personal exertions. This, we rejoice to see, has been heartily recognised by the Trustees, and the "sincere good wishes" which they have offered through Lord Dillon, as chairman of the Standing Committee, will be shared by the many who have, in one way or another, come into contact with Sir William Flower.

The Directorship of the Natural History Museum

The question of Sir William Flower's successor has for some time past exercised the minds of British naturalists, and the names of many more or less eminent men have been mentioned in that con-

nection. Sir William's retirement, we may remind our readers, creates two vacancies, namely, the Directorship of the Natural History Section of the British Museum, and the Keepership of the Zoological Department, the duties of the latter post having been taken over by Sir William on the retirement of Dr Günther. For the present Dr Henry Woodward is acting as Deputy-director, while the work of the Zoological Department is presumably in the hands of the Assistant-keepers. Yet a third question suggested itself to many, owing to the adoption by the Principal Librarian, under a recent Treasury scheme, of the title Director. This was supposed to show that the Trustees wished to bring the whole of the British Museum more immediately under one head than had been the case since the removal of the natural history collections from Bloomsbury. These fears were confirmed by a prompt official *démenti*.

Against any action that might lessen the independence of the Natural History Museum an energetic protest was soon raised, and a memorial signed by many leading men in various branches of science, and by others, was stated by the public press to have been addressed to the Trustees. The memorialists considered that the principal official in charge of the natural history collections should not be subordinate to any other officer of the Museum; he should meet the Trustees and represent them before Her Majesty's Treasury as the responsible head of a department and not as a subordinate. It is clear, the memorialists pointed out, and their contention was emphasised by a leading article in the *Times*, that the interests of the Departments at Bloomsbury are totally different from those of the Natural History Museum, and that the special knowledge and sympathies and individual museum experience that fit a man for the post of Principal Librarian militate against his caring adequately for the wants of natural history. The *Times* further observed that the proposed action would deprive naturalists of one of those very few posts "to which they might reasonably look forward as a reward for study and research; and we all know that . . . the prospect of reward may serve to keep an able man steadfast to a pursuit which he might otherwise be tempted to forsake for some other and more promising mode of activity." The inducements to enter the service of the Natural History Museum are, it has been stated, none too high from a pecuniary point of view, and the suggestion appears to be that the Trustees are far more likely to obtain a good class of assistants in future if they let it be seen that the highest posts are not excluded from those who have gained knowledge and experience in a long and devoted service.

Other opinions of interest and originality were elicited by the newspaper discussion: such as, that a botanist is not a naturalist, that Mr Du Cane Godman is merely a collector of insects, that a

voyage to Bering Straits does not necessarily fit a man for the administrative work of a large government department, that the Primate cannot tell a Painted Lady from a Camberwell Beauty, that a man who cannot teach the Trustees how to suck eggs is not fit to direct a Museum of Natural History. However these things may be, the recommendations of the Standing Committee have been laid before the three Principal Trustees, the three have adjourned the discussion, and criticism of their action must therefore be reserved for our next number.

The Museum of 'Practical' Geology

We learn from *Nature* that 500 Fellows of the Geological Society signed a memorial to their president and council protesting against the transference of the Museum of Practical Geology to South Kensington, since "removal of the collections would seriously impede the progress of science, especially on its economic side." The Council did not see its way to comply with the request of the memorialists, that it should address the Government on the subject; indeed, it expressed the opinion that the question of the removal required more consideration than it appeared to have received. Certainly this laying stress upon the 'economic side' of the question is a trifle ridiculous. As a well-informed article in the *Builder* of June 25th points outs, economic geology is the one thing that is lacking at the Museum of Practical Geology. The collection of building-stones is very incomplete, and even that is unaccompanied by the necessary particulars. "The clay-working industry," says our contemporary, "is not much advanced by the miserable show at present arranged in the Museum." Agriculture, and even mining are but indifferently attended to. If a museum "with the special object of illustrating the applications of geology to the useful purposes of life" is a desideratum, and we do not for one moment deny that it is, then let us have one, and let it be in the place that is most convenient to engineers, architects, well-sinkers, medical officers, and such practical men. But all this has mighty little to do with the fine, stratigraphical series of British fossils, of which only a small portion is named and exhibited at Jermyn Street, and that in a manner to reflect credit on the palaeontologists of a past generation.

French 'Protection' of Fossils in Madagascar

We learn from the July *Geographical Journal* that the *Politique Coloniale* of May 25, 1898, publishes a circular issued by the French Governor of Madagascar, ordering the local officials in this colony to prevent any but Frenchmen from collecting fossils in the island. No one is to be allowed to collect fossils unless he be provided with a special licence from the Governor; and this will only

be granted to his fellow-countrymen. Further than this, complaint is actually made that foreign scientific men have already secured too many of the fossil treasures of the island. We wonder whether the naturalists of France, official and otherwise, have been consulted on this subject, or whether it is merely the order of a politician ignorant of the methods of scientific men. It will, indeed, be strange if the enlightened Government of France, which does so much for the promotion of research in foreign lands, should allow this policy to be pursued in its latest dependency. Natural science has hitherto known no division into nationalities. On the contrary scientific work confers a free-masonry on those who pursue it, and is the strongest force towards the federation of the world. It should not be turned into a cause of division. It is not long since the Colonial Government of Mauritius paid for excavations in that island to exhume the fossil remains of birds. These were investigated and described by our own ornithologists in the University of Cambridge, and were faithfully returned by them to the President of the Excavation Committee for preservation in the Museum of the Colony. Among other treasures then obtained was the finest known skeleton of the Dodo. This, however, along with other specimens, was eventually removed from Mauritius to the Museum of Natural History in Paris, where it is now one of the greatest ornaments. Surely a nation which can accept foreign courtesy in such a manner can ill afford to countenance such petty spite as that displayed in the manifesto of the Governor of Madagascar. We trust it is enough to draw the attention of French naturalists to the subject, that they may use their influence in a matter which ought to be beyond all political considerations.

An American Pirate

In our June number we published a specially written article, entitled "A Geographical Commemoration: Vespucci, Deschnev, and Vasco da Gama." This article has been reprinted in the *Scientific American* for July 2, 1898 (vol. lxxix. p. 8). A few verbal alterations have been made, causing the article to appear as though prepared by the staff of the *Scientific American*. Five lines in the article are, it is true, placed between quote-marks, and ascribed to *Natural Science;* but no one would imagine from this that the whole article was lifted bodily from our pages. What makes this treatment worse is, that we acknowledged quite fairly and frankly that some of our statements concerning Vespucci were taken from an account in our contemporary. This is not the first time we have had to complain of similar piracy on the part of the *Scientific American*.

Scenery and the Poets

The Romanes Lecture for 1898 was delivered in the Sheldonian Theatre at Oxford on June 1, by Sir Archibald Geikie, who took for his subject "Types of Scenery and their influence on Literature." This has now been published, at the price of two shillings, by Messrs Macmillan & Co. The subject of the lecture is less than its title. Scenery is limited to Great Britain; its types are defined as Lowlands, Uplands, and Highlands; and by 'literature' we are for the nonce to understand 'poetry.' Even within these somewhat narrow bounds, we are not sure that Sir Archibald has made the most of his theme. It was a delightful lecture, full of pretty word-painting and apt quotation, with a geological flavour deftly introduced so as to make the listener think that there was a foundation of abstruse science, and that this particular branch of science was perfectly charming. But as an essay, as a contribution to serious thought on the subject, its tenuity approaches transparency. A poet, we gather from the lecture, describes what he sees, and draws his images from his surroundings. Cowper depicts the valley of the Ouse; Thomson turns from "the living stream, the airy mountain, and the hanging rock" of his native Border to Hagley Park and the "sleep-soothing groves" of the south of England; Burns sings the "banks and braes o' bonnie Doon" and "the bonnie banks of Ayr"; Wordsworth, fortunately for English literature, lived in the highlands of the Lake District, and introduced to us mountains and lakes and sounding cataracts, "clouds, mists, streams, watery rocks and emerald turf." But all this, though we may not have heard it expressed in such felicitous language, we knew before.

The most striking and original passage in the essay is that which applies the scenic method of criticism to Macpherson's "Ossian." "The landscape," we are told, "belongs unmistakably to Western Argyleshire. Its union of mountain, glen, and sea removes it at once from the interior to the coast. Even if it had been more or less inaccurately drawn, its prominence and consistency all through the poems would have been remarkable in the productions of a lad of four-and-twenty, who had spent his youth in the inland region of Badenoch, where the scenery is of another kind." "It is not that in Ossian, highland landscape was deliberately described, but it formed a continually visible and changing background. The prevalent character of the whole range of scenery in the region, and the general impression made by it on the eye and mind, were so vividly conveyed that no one familiar with the country can fail to recognise how faithfully the innermost spirit of the West Highlands is rendered." This is strong as well as new evidence in favour of the authenticity of at least a large proportion

of the Ossianic poems. Moreover, as to the question whether the English or Gaelic is the original, Sir Archibald says, "There can be no doubt that on the whole the Gaelic is more vivid and accurate in the description of landscape than the more vague and bombastic English of Macpherson."

In Ossian we are dealing with true nature-poetry; equally unsophisticated are the Border ballads, and it is in the essential character of such folk-poems rather than in the allusions, descriptions, and similes of more cultured and artificial poets, that search should first be made for the influence of scenery and topography upon literature. If scenery have any influence upon the subject-matter, the form, or the niceties of style, that influence will be discovered more readily by comparing groups of writers from different regions (such as the Lake School, the Attic Dramatists, the Norwegian Novelists) and estimating their common characters, rather than by contrasting individual writers. There are more important subjects that call for investigation, and yet we should not be sorry if Sir Archibald Geikie's eloquent lecture led to further essays on these lines. Literature to-day has so largely become a matter of "words, words, words," that the mere suggestion of a possible connection with external nature cannot fail to do good. And if our stylists can be brought to look at the larger conception of nature resulting from modern science, or if our scientific students can be led to look on literary form with less contempt, then it is possible that the literature of the future may share in the remarkable progress that has already fallen to the lot of modern science.

Models of Multipolar Cells

Professor A. L. Herrera, to whose curious experiments on the production of artificial simulations of organic structures we referred in February last (vol. xii., p. 74), has sent to us from the National Museum at Mexico an account of a new result he has obtained. He noticed accidentally that when a greasy solid is lightly dabbed with a brush dipped in a viscous liquid, the liquid rapidly assumes the form of a network of multipolar ganglion cells. He sent us along with the letter a shallow tin box, the bottom of which, on its inner surface had been greased with butter and then had received an application of some coloured viscous fluid. This fluid had assumed the configuration of a group of multipolar cells, and when it reached us, still retained that appearance. Dr Herrera wishes to correlate this observation with the older experiments upon the artificial production of nervous simulacra out of myelin, as described in Robin's treatise on the microscope (Paris, 1871, p. 569). We are not prepared to go so far as the Professor in believing that such experiments throw light upon histogenesis, but they are interesting and ingenious.

Synthetic Protoplasm

Professor Herrera also sent us a letter containing an account of some experiments he has made on what he calls a 'synthetic protoplasm' made by him by mixing pepsine, peptone, acetic fibrine, oleic acid, soap, sugar, extract of bile, carbonate of soda, lime, and of ammonia, lactate of lime, phosphates of lime and magnesium, sulphates of lime and iron, and chloride of sodium. When this compound is placed in water and examined under the microscope, violent diffusion currents are set up, and streaming movements of a very active kind last for a few minutes. When this is over, the addition of a trace of liquid ammonia renews the activity, which lasts for several hours. These movements he rightly attributes to the liberation of gases by the particles of the compound, and he compares with this the liberation of carbonic oxide by living protoplasm, suggesting that in the discharge of that gas lies the secret of the streaming movements of protoplasm. Professor Herrera was good enough to send us a small quantity of his compound, and on this we have successfully repeated some of the experiments he describes. The little mass was particularly active in water with a trace of ammonia: diffusion currents, movements of the whole mass, and the protrusion of pseudopodia-like processes occurred. We found, however, that the resemblance to protoplasm was destroyed from the fact that the mass did not retain its coherence; small masses were constantly discharged from its surface, and it seemed to melt away in a comparatively short time. In this respect it compared unfavourably with Bütschli's foams, and we imagine that, although Dr Herrera has imitated with considerable success the very complicated chemical composition and consequent instability of protoplasm, he has not been so successful with its structure. On the other hand, it is possible that the substance, in its postal journey from Mexico, has deteriorated. We think that he has begun a most ingenious line of experiment, and wish him all success in his further attempts.

The Phyto-Plankton of the Atlantic

On May 12, Mr George Murray and Mr V. H. Blackman presented to the Royal Society their observations on a year's work in collecting phyto-plankton along a track from the Channel to Panama, carried out by Captains Milner and Rudge, and also during one voyage to Brazil by Captain Tindall. They also gave the results of their own observations on living material at sea. The material was obtained by the pumping method described in our June number (vol. xii., p. 367).

One of the objects of the authors' work was to determine, if possible, the nature of the microscopic and little understood objects known as coccospheres and rhabdospheres. In the present paper

they describe the minute structure of the calcareous plates or coccoliths and rhabdoliths, and record the existence in the coccospheres of a single central green chromatophore, separating into two on the division of the cell. They regard Coccosphaeraceae as a group of Unicellular Algae, and they define the group with the limits of its genera and species. The coccospheres and rhabdospheres from the surface are compared with those of the deep-sea deposits and their identity established. They are also compared with geological coccoliths and rhabdoliths from various beds, and many objects regarded by geologists as true coccoliths and rhabdoliths are rejected.

A large number of new Peridiniaceae were discovered and are formally described and figured. No specific diagnoses of marine Peridiniaceae have previously been published, authors of species having depended on figures, and, at most, a few words of description. No doubt the present systematic treatment of the subject will conduce to greater order in the group. The authors record the occurrence of all the forms in seven tabular statements, one for each collecting voyage.

A study was also made of the species of *Pyrocystis*, of which a new one is described. The facts here recorded tend, in the opinion of the authors, to confirm the view originally expressed by Sir John Murray, its describer, that it is an unicellular alga, though doubts had been entertained of the accuracy of this opinion by several biologists.

The Missouri Garden

The ninth annual report of the Missouri Botanical Garden, issued by the director, Professor Trelease, has just reached us. As in the case of previous issues, it contains the garden reports, embellished with photographic reproductions of features of interest, and a number of scientific papers, several of which deal with those dry-loving tropical plants for the growth of which the garden is eminently suited. One of these papers, a revision of the genus *Capsicum* (which includes the Chili and Cayenne peppers), with special reference to garden varieties, by H. C. Irish, studies an interesting example of the assumption of numerous forms by plants which have been long cultivated. The genus, which is an ally of *Solanum*, the potato and tomato genus, evidently originated in the tropics of America, whence it was probably first brought to Europe by Columbus, and is now widely spread in the Old World tropics. Linnaeus made four species, and the number of specific names has since so increased as to reach at the present time nearly one hundred, of which the Index Kewensis recognises over fifty as good. Asa Gray, however, suggested that these might perhaps all be reduced to two, and Dr Sturtevant, who made a special study of the genus, collecting and

cultivating a great many kinds, also came to the conclusion that a majority of the published species were merely varietal forms, and that the number which botanists would hold as good species would be very small. Dr Sturtevant recently gave the whole of his material, including herbarium-specimens, drawings, and notes to the Missouri Garden on condition of the ultimate publication of the results of further study in the form of a monograph. This has now been done by Mr Irish, who has confirmed Dr Gray's suggestion as to the existence of but two species. One, *Capsicum annuum*, is herbaceous or slightly shrubby, and of annual or biennial duration; the other, *C. frutescens*, is a shrubby perennial. The former is responsible for the great majority of the cultivated forms, which are distinguished by characters based mainly on the shape, size, and erect or pendent position of this fruit, and in the form of the calyx. Many of the forms are depicted in the twenty-one plates which accompany the monograph.

Among the other papers are a revision of the American duck-weeds (*Lemnaceae*) by C. H. Thompson, and notes on a willow, *Salix longipes*, from the South-Eastern States, by Dr Glatfelter, both of which have been previously issued. There are also notes on species of *Agave*, *Cactus* and *Yucca* by various authors, and a list of Cryptogams collected by Mr A. S. Hitchcock in the Bahamas, Jamaica, and Grand Cayman. Among the last mentioned plants Mr J. B. Ellis finds fourteen species of fungi belonging to the group Pyrenomycetes, of which nine are new. A fungoid disease which had attacked the leaves of certain palms belonging to the same genus as the date (*Phoenix*) is described by Professor Saccardo as caused by a new species.

In addition to the five plates which refer to the Garden, there are fifty to illustrate the various scientific papers.

Wasted Wealth

It is often the case, even in these days of technical education, that for want of a little elementary knowledge the wealth which lies at our feet is entirely overlooked. A remarkable instance of this is furnished by the chemical manure factories of Lincoln. These supply a large proportion of the artificial manures used in the kingdom, yet the whole of these commodities are manufactured out of imported material. A well-informed writer in the *Lincolnshire Chronicle* has lately pointed out that all the expenses of importation might be saved.

"In view of the great stores of mineral phosphates that lie in the rocks on which Lincoln city is built, it is," he says, "inexplicable that our factors should go to the trouble and expense of bringing in all this foreign material. For those interested in this subject a visit to Handley's Brickyard, just below Swan's Pit, will prove a valuable object lesson. Interstratified with what are known as the Marlstone

and the Shale, lie several seams of so-called coprolites or nodules of phosphates of lime. The seams vary in thickness from three inches to eighteen inches, and all are formed of these nodules closely compacted. We have had an opportunity of assisting at an analysis of these nodules, from which it was demonstrated that not only are they remarkably free from iron, but that they are ten per cent. richer than are the imported Belgian samples. It must not be supposed that these seams are peculiar to Handley's Pit only—they extend uniformly throughout the middle hills of Lincolnshire, and are exposed in most of the pits from Lincoln to Grantham. If properly exploited the phosphate industry in Lincolnshire might be made second only to that of its great iron mines and foundries."

Humber Mud

There is mud in the Humber estuary, mud and sand, and a great deal of it. Where does it come from?

This is the question that the Hull Scientific and Field Naturalists' Club set itself to discuss on April 13th, the discussion being opened by its energetic secretary, Mr T. Sheppard. The usual reply that the mud comes from the tributary rivers, Ouse, Trent, and Hull, is soon found to be inadequate. The mud is accumulating so rapidly that the material brought down by the rivers is insufficient to account for it. Not only sand and mud banks but dry land is continually being formed. Reed's Island, between North and South Ferriby, was, some twenty-five years ago, comparatively small, with a plot of grass on which a few cattle were reared. Now it is hundreds of acres in extent, has an enormous number of cattle grazing upon it, and is still growing. As for the accused rivers, their waters are comparatively clear and hold but few particles in suspension. Such detritus as they do transport is for the most part redeposited on their own banks, or in the alluvial flats so characteristic of the Ouse and Trent. It is, however, noticed that the water near the mouths of these rivers is far more muddy when the tide is flowing up them; and this suggests that the mud may be brought into the Humber from the sea.

The waters of the North Sea are continually washing particles of rock, sand, and mud in a southerly direction, and slowly but surely the material on the Yorkshire coast is travelling southward. It never travels in a northerly direction. As is well known, the beach around Flambro Headland is strewn with masses of chalk of all sizes, which have been dislodged from the cliffs. These can be seen in plenty in Bridlington Bay and further south, though naturally getting less plentiful as they get Humber-wards. Hardly any chalk boulders are found north of the headland. This proves that the beach-material travels southward. Now the cliffs of the Holder-

ness coast are made up of soft glacial clays, capped in one or two places by lacustrine deposits of small extent. They vary in height from 10 to 50 feet, and at Dimlington reach over 100 feet. Mr J. R. Boyle has shown on historical evidence, and the Rev. E. M. Cole and others have proved by direct observation, that the whole of the cliffs from Bridlington to Spurn are being eroded at an average rate of about 7 feet per annum. The whole of the eroded material must be gradually, or in some cases quickly, converted into gravel, sand, and mud, and carried southwards. A large quantity of this material is carried past the Humber mouth and is gradually silting up in the Wash and off the Lincolnshire coast. At the same time a deal of it must be brought into the Humber at each tide; and when the winds are the strongest, and the erosion most severe, the inrush of water into the Humber is likewise the greatest. This water brings with it the cliffs in a modified form. It would appear, therefore, that it is from the coast that the bulk of the material suspended in Humber waters is derived.

It does not follow that the mud now in suspension in the Humber is the result of one or two tides. The particles may have been accumulating during several months, and undoubtedly pass and repass a particular point several times a week. Consequently, when the rivers flowing into the Humber are swollen with flood waters, and are swift, the muddiness observed near their entrances to the estuary is not necessarily due entirely to the additional material which they have brought down, but is more likely to be owing to the sediment in the Humber being stirred up.

Absolute confirmation of this theory as to the origin of the Humber mud, such as might under other circumstances be afforded by microscopic examination of the mud-particles, is not to be obtained, since the particles brought down by the rivers are precisely similar to those found in the cliffs of the east coast. Not only are the boulders in those cliffs formed of rocks similar to those eroded by the rivers near their sources; but in their lower reaches the rivers traverse boulder-clay areas.

Slugs

There are before us some papers dealing with slugs, and written by Mr Walter E. Collinge, one of the few British workers who have turned their attention, with any persistency, to this branch of malacology.

The forms included among slugs are not of necessity near relations, nor are they, as some might imagine, sharply separated off from types with well-developed shells. Nevertheless, slugs as a whole present the same difficulties, and require to be approached from the same point of view.

The student of slugs obviously cannot be a mere conchologist, but one of two temptations may beset him according as he is or is not an anatomist. In the latter case, he may be led to attach undue importance in descriptive work to colour variations, occasionally making one species out of two, but more often splitting up one into several, and possibly naming 'varieties,' which may be based merely upon such phases of coloration as succeed one another during the life-history of a single individual. The anatomist, on the other hand, may be inclined to consider as diagnostic of species points of internal difference so small as to suggest that the describer's wish was father to his thought. And this is especially the case when little or no outward differentiation is obvious. Of course, in these days when 'physiological species' are recognised, there is nothing inherently improbable in the assumption that there are forms alike without yet not within, provided that some additional evidence is forthcoming as to varying habits or development. It seems reasonable, however, to expect something of the latter sort to be brought forward before we are asked to recognise 'anatomical species' as good ones.

Mr Collinge is an anatomist, and in his paper "On some European Slugs of the genus *Arion*" (*Proc. Zool. Soc.*, 1897, pp. 439-450, pls. xxix.-xxxi.) he brings forward evidence as to "the constancy of anatomical characters" so far as the genital organs are concerned, these being the parts in which specific differences are mainly sought. The testimony is based upon the small number of variations found in a large series of specimens dissected, and belonging to the same two species of *Arion*. That the form of generative organs in the particular genus considered presents but little modification throughout the species is a well-known fact, and, even if this were not so, a comparison between fig. 3 of *A. subfuscus* and fig. 12 of *A. hortensis* would suffice to show it. To bring forward this 'constancy' in *Arion* as militating against Cockerell and Larkin's belief in the specific identity of several forms of *Veronicella*, which differ anatomically, is but lost labour. So far as *Arion* itself is concerned, the small number of slight variations noted by Mr Collinge (*i.e.*, 26 out of 1223) may serve as an excuse for increasing the number of species when more marked differences are from time to time detected.

Mr Collinge, indeed, in the paper under discussion, reserves his *Arion hortensis*, var. *caeruleus*, which differs more markedly from the typical *hortensis*, and raises it to specific rank. We note that *A. hortensis* is figured with but one vestibule, while *caeruleus* has two: two, however, are shown in an unpublished drawing of the former by the late Charles Ashford now before us, while Mr Collinge's fig. 12 contradicts his own statement, that in no species of

the *A. hortensis* group is the lower portion of the free oviduct much larger and more globose than the upper. Again, in the paper, the validity of *A. fuscus*, externally very much like *A. subfuscus*, is admitted by Mr Collinge who previously rejected it. This is not to be wondered at since, in another paper on four species of the same genus, published about the same time in the *Journal of Malacology* (vol. vi., pp. 7-10, pl. ii.), almost the only evidence of external difference between *A. empiricorum*, *A. ater*, and *A. lusitanicus* given by Mr Collinge is a short series of measurements pointing to the fact that the second species is larger than the first.

Of course it is probable that our islands contain more species of *Arion* than many of us used to imagine, and Mr Collinge has done much to arouse us to the truth about them; but, before all the species which he and his continental friends would give us are recognised by thinking malacologists, still more careful and detailed work must be done upon the genus. We have been so busily criticising the weak points in the paper that we have only hinted, by quoting a few figures, at the prodigious amount of work which it must have entailed. There only remains for us to express the hope that other workers will come forward and supplement what is a valuable contribution, from an anatomical point of view, to our knowledge of the genus *Arion*.

In the other paper we have "the description of two new species of slugs of the genus *Parmarion* from Borneo" (*Proc. Zool. Soc.*, 1897, pp. 778-781, pl. xliv.). One of these the author considers intermediate between the genus in which he places it, and *Microparmarion*; in fact it leads Mr Collinge to the conclusion that no line can rightly be drawn between the two genera.

American Isopods

Mr J. E. Benedict on March 24 published a paper on "The Arcturidae in the U.S. National Museum" (*Proc. Biol. Soc. Washington*, vol. xii., pp. 41-51), in which there are several interesting features. He describes eight new species, and of each of them gives a good and intelligible 'habitus-figure,' an adjunct without which the most lucid description of a new form is usually difficult and wearisome reading. Within the last twelve years the number of species in the genus *Arcturus* has been raised from five to twenty-five. Mr Benedict has therefore rendered a kindly service by supplying a key to this rapidly increasing group. As often happens, when new species appear, some of the old generic distinctions have to disappear. Mr Benedict finds, for example, that in some species of *Arcturus* the fingers are not simple, but biungulate as they are in *Astacilla*. It appears, too, from his descriptions that *Arcturus* cannot invariably be distinguished from

Astacilla by having the flagellum of the second antennae more than four-jointed. Nature lures us on by showing species after species in which the joints are more than four, and, when we are purely satisfied with our generic character, brings to view *Arcturus multispinis*, in which the flagellum in question is single-jointed. When *Astacilla* has established a character for preferring shallow water, Mr Jules Bonnier describes the remarkable *Astacilla giardi* from a depth of more than 500 fathoms, and now from 1825 fathoms comes Mr Benedict's *A. caeca*. Mr Beddard has pointed out that the genera *Arcturus* and *Astacilla* "form almost the only exception to the general statement that the deep sea Isopoda are blind," and now Mr Benedict's last-mentioned species comes as an exception to this exception, being a blind Isopod from the deep sea.

In a second paper, simultaneously published in the same *Proceedings* (vol. xii., pp. 53-55), Mr Benedict describes two new Californian species, both of which he assigns to the genus *Idotea*, relying for the limits of that genus on the monograph of the Idoteidae by E. J. Miers, published in 1883. It is rather surprising that he takes no notice of the far more recent discussion of this family by Mr Adrien Dollfus in the *Feuille des Jeunes Naturalistes*, November 1894, and February 1895. Few students who have read the papers by Mr Dollfus will be ready to retain Mr Benedict's two new species in one and the same genus.

Miss Harriet Richardson, also in the same publication (xii., pp. 39-40) describes and figures a new species of *Æga*, closely related to *Æga tridens*, Leach. The discriminating characters do not seem to be all of them quite convincing. One of these depends on the number of joints in the flagella of the antennae, the new *Æga ecarimata* having on the second pair ten joints, while Leach's species has nineteen. It is true that the number nineteen is assigned to it by Schiödte and Meinert, but Bate and Westwood say that the number is about twelve, and it may be taken for granted that there is no fixity in this respect to depend on among specimens of different ages and different sizes. The relative length and breadth of the body is likely to prove an equally unstable character. The new species is said to be more than thrice as long as broad, and the figure given agrees with this measurement, but so does the figure of *Æga tridens* given by Schiödte and Meinert, though that drawn by Bate and Westwood is of a more portly habit.

Under present conditions of human existence the scattering of scientific information remains unavoidable, so that one can merely note, without astonishment or disapproval, that one new species of *Æga* obtained by the U.S. Fish Commission steamer 'Albatross'

is described by Miss Richardson in the *Proceedings of the Biological Society of Washington*, and that four new species of the same genus obtained by the same vessel are described almost at the same time by Dr H. J. Hansen in the *Bulletin of the Museum of Comparative Zoology* at Harvard College. Of the high merit of all Dr Hansen's zoological work mention has been too recently made to need further comment on this occasion. In regard to Miss Richardson's excellent contributions to our knowledge of the Isopoda, the suggestion may be diffidently hazarded that researches reported in a collected and connected form are now-a-days more acceptable than isolated descriptions.

Some Mexican Birds

In a short paper published in the *Proceedings of the Biological Society of Washington* (vol. xii., pp. 57-68; March 24, 1898) Mr E. W. Nelson includes a critical examination of the long-nailed partridges, for which Mr Ogilvie-Grant established the genus *Dactylortyx* in 1893 (cf. Cat. Brit. Mus. xxii. p. 429). Mr Ogilvie Grant was only able to include a single species, *D. thoracicus*, under this genus; but Mr Nelson decides that Mr Ogilvie Grant united two distinct species, of which he supplies the distinguishing characters. He also describes two new species of long-nailed partridges, both obtained in Mexico. Mr Nelson has discovered several other species and sub-species of birds in Mexico. Of these, perhaps the most surprising novelty is the Sinaloa Martin (*Progne sinaloae*), procured upon the western slope of the Sierra Madre, between 2500 and 4000 feet altitude. Oddly enough, this new species from Northwest Mexico is closely related to the pretty Caribbean Martin (*Progne dominicensis*) which is peculiar to the West Indian Isles. A good figure of the latter will be found in Sharpe and Wyatt's "Monograph of the Hirundinidae," vol. ii. plate 91.

The Black Kite

Count Arrigoni Degli Oddi is one of the most enthusiastic of the younger generation of Italian ornithologists, and has recently published several excellent papers on the birds of his country. An essay just issued by him, at Venice purports to be a notice of the nesting of *Milvus migrans* in the province of Verona, but it is, in point of fact, almost a life-history of the Black Kite. The author supplies dates for the arrival and departure of this hawk in and from Verona for a term of fifteen years, from which we learn that it reaches its summer quarters in March, and leaves for Africa again in August, or at the commencement of September. The duties of incubation are performed by the female bird, and occupy from eighteen to twenty days. The old kites are devoted to their

young ones, which, we learn with regret, are fed largely upon small chickens. But, lest it be supposed that this kite should be banned as vermin, we hasten to observe that its diet is very varied, including worms, small crustaceans, dragon-flies, grasshoppers and other insects, frogs, toads and water-newts, lizards, snakes of two species, and five species of fish; not to mention small mammals, such as moles, shrews, mice, and (last, but not least) land-voles, which are the bane of agriculturists.

An Alga parasitic on Ophiurids

The occurrence of Algae in intimate connection with the bodies of living animals is well known. Such are the yellow cells (*Zooxanthella*) of radiolarians, the green alga, *Zoochlorella*, found in many infusorians, in *Spongilla*, *Hydra*, in various rhabdocoele turbellarian worms, and so forth. But in all these cases the relation is one of symbiosis, on the give and take principle. Lately, however, Dr Th. Mortensen has described in *Videnskabelige Meddelelser* (1897, pp. 322-324) a species (probably a new one) of the green alga *Dactylococcus* infesting the brittle-stars *Ophioglypha texturata* and *O. albida*. It forms dark-green patches on both disc and arms, on both upper and under side, sometimes in small excavations in the calcareous plates. Its peculiarity is that it eats away the skeletal substance and, apparently, does not pay for it. It is a true parasite.

Paper

Since every scientific man should believe that his work is of permanent value, and should avoid publishing anything in which he does not so believe, it becomes of some interest to him to see that the paper upon which his articles are printed is of a quality warranted to last. Therefore we recommend our readers to consider with care the valuable report printed in the *Journal* of the Society of Arts for May 20, in which a Committee appointed for the special purpose, give the results of their investigation into numerous printing papers. The disintegration of the fibre of papers is, as a rule, referable to acidity in the case of rag papers: while, in those made from wood-pulp it is due to oxidation, and is accompanied by an alkaline reaction. The discoloration of papers is proportional to the amount of rosin which they contain. The Committee, therefore, advocates the following specification as that of a reliable paper :—"Fibres: not less than 70 per cent. of fibres of the cotton, flax, and hemp class. Sizing: not more than 2 per cent. rosin, and finished with the normal acidity of pure alum. Loading: not more than 10 per cent. total mineral matter (ash)."

I

Some More Rowing Experiments

IN the autumn of 1895 the author designed an indicator for recording the work done in a stroke of rowing.[1] The apparatus replaced the front thowl of the rowlock, and consisted of a plate turning with the oar, which pressed against it, about an axle to which it was connected by means of another plate pivoted to the first.

The pressure of the oar tended to move the front plate back towards the axle, this motion being resisted by a spring. The movements of the plate, and consequently of the oar, were recorded as a diagram on a horizontal card by a pencil connected with the plate. This diagram afforded the information given by the familiar steam-engine indicator diagram, and gave measures of the horse-power of oarsmen, as well as interesting information with regard to the way in which the work was done, showing the great differences that existed between the style and stroke-forms of different oarsmen.

The successful working of this simple instrument encouraged the author in 1896 to attempt another, obviating the shortcomings of the first. This latter, in addition to various mechanical imperfections, had the disadvantage of giving the diagram in curvilinear co-ordinates, necessitating laborious measurement and reduction before stroke form and work done could be estimated.

Further, in testing a steam-engine it is customary to take several diagrams during a run, by changing the card on which the figures are being drawn. In a boat this is impracticable without stopping the rowing. It will be seen that an important part of the later machine is the automatic winding apparatus, whereby the card changes itself. The author was recently interested in having his attention called to a similar device which was being introduced for the steam engine.

The general principle of the former indicator, that of recording the movements of a plate turning with the oar and pressed forward by a spring, has been adhered to. In the photographs (Plate I.) and diagram illustrating the action of the pencil and spring (Fig. 1) the face *A* moves with the oar, and is connected with the axle *B*

[1] For a description of the instrument and results obtained, see *Natural Science*, vol. viii., pp. 178-185, March 1896.

(which screws on to the rowlock in place of the thowl) by means of the link *C*. This junction piece, which is strongly braced to avoid flexure, rotates round the axle in an adjustable bearing, and is pivoted to the face at *D* in dead centres. The spring is placed between *E* on the back part of the front plate, and a nut at *F* on a core, fixed at *f* to the link.

The part of the instrument in which it differs essentially from the earlier one is the recording mechanism, in which the diagram is obtained practically in rectangular co-ordinates, and every fifth stroke is automatically indicated for a period up to 500 strokes.

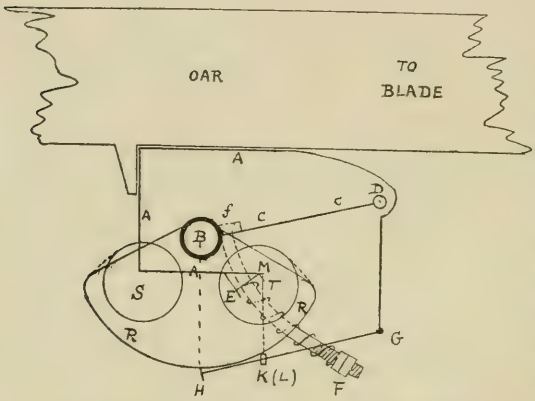

Fig. 1. Diagram illustrating action of Pencil and Spring.

The diagram is recorded on a drum (*R*) having its axis along the axle. The strip of 'metallic paper' on which the diagram is drawn winds off the cylinder *S*, over the drum on to the cylinder *T*, and is held in position by an elastic thread which winds off a pulley on *T* on to a similar pulley on *S*.

The pencil attachment (*HGL*) moves as a whole about a vertical axis *G* fixed to the face, while the arm *GH* can move about a horizontal axis fixed to the main attachment, which keeps it in a vertical plane with the pulley *L*. *G* is so placed that (Fig. 1) *BDGH* is a parallelogram (assuming for the moment that *H* has no vertical movement), and, consequently, if *H* remains on the drum while the face moves parallel to itself towards the axle, it will stop in the same position. The pencil (*H*) (a brass point) is pressed upwards by a spring, and is held down by a chain which passes over the pulley *L* and is fixed to the face at *M*.

If the front face moves backwards parallel to itself, *G* and *M* participate in the movement, while *H* rests on the drum owing to the spring, which, keeping the chain tight, pulls *L* towards *M*. Consequently *M* approaches *L*, and some of the chain passes to the vertical part, and allows the pencil to move upwards. In this way a pressure line is drawn, which is a circle about *G* as centre. The maximum error introduced into the position of the oar by regarding this as a straight line is about 0·5°,[1] while the line of no pressure

$$^{1} = 3\left(1 - \cos \sin^{-1} \tfrac{1}{2}\tfrac{3}{4}\right) \times \frac{360}{2\pi \times 2}.$$

produced by simply turning the oar, is a horizontal circle, which becomes a straight line when the diagram strip is unwrapped.

At the conclusion of a stroke, when the pencil has reached its extreme position to the right, the oar leaves the face and rests against the opposite thowl, only touching the instrument at the rounded end of the face to carry it into position for the next stroke. During this movement the pencil travels over the base line, and a lever N engages a spoked wheel O, which gears into the right hand cylinder and winds the strip into a new position. As the winding ceases, a cam-wheel P is rotated; this drives a plate attached to the bottom of the drum outwards, and thus lifts the pencil off the strip and the winding lever out of gear.

During the stroke the wheels are not engaged, since the levers share the backward movement of the face when the spring is compressed and pass clear of the wheels. While the cam holds the plate out no diagrams are drawn and no winding goes on. Each forward swing carries the cam wheel forward one tooth, until at the fifth stroke the pencil again reaches the paper, another diagram is drawn and the strip is once more wound. Two cords place the starting and stopping of the record under the control of the coxswain. The upper part of the instrument is protected from splashing or rain by a cover.

Knowing the diameter of the drum, and the strength of the spring, the diagram gives a result in pounds pressure per degree of turn of the oar. In order to express the result in foot-pounds of work, the ratio of the pull on the handle of the oar to the corresponding pressure on the rowlock must be known, and the distance through which the handle moves per degree of turn. These two constants are found from the in and outboard dimensions of the oar, and the position of the centre of pressure of the water on the blade.[1] The spring was calibrated in the Millard Laboratory, Oxford, by kind permission of Rev. F. J. Jervis-Smith. With the oar and spring used in the experiments, a height on the diagram of 1″ represents a pull of 220 pounds, and 1 sq. inch represents 377 foot-pounds.[2]

When the indicator is fully open it is usually convenient to have the spring under some initial compression. This is estimated by noting the turns given to the nut F after it has just touched the spring, and allowed for by measuring heights from a new base-line 0″·06 per turn below the actual one. Experiments in calibrating the spring showed that one end did not completely 'bed' itself until some pressure was exerted. The error due to this is practically eliminated by raising the base line 0″·03.

[1] Neglecting the moment of inertia of the oar about its button. See Appendix I.
[2] See Appendix I.

In order to prevent the necessary length of the strip being excessive, the diagrams are made to overlap (Fig. 2). If the strokes are flat at the top it is frequently not easy to determine at sight which is the 'finish' corresponding to a particular 'beginning.' This is, however, discovered at once by measuring the length of the first or last stroke, from which the approximate distance of the 'finish' from any 'beginning' is known. A stroke thus individualised can then be measured up. The measurements taken are:—

(1) Length of stroke in degrees: which is afterwards reduced to distance moved through by the handle of the oar.

(2) Greatest height of diagram.

(3) Area, measured with an Amsler planimeter.

(4) The shape of the curve gives the style of stroke.

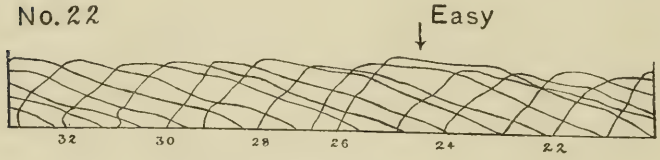

Fig. 2. Part of an Indicator Card (two-thirds actual size).

To determine the style of an oarsman under any particular circumstances, the idiosyncrasies of the strokes are eliminated by superposing several diagrams (Fig. 3), and then drawing a mean line through the result. In this way the 'Characteristic Diagram' is obtained. Similarly, in finding an oarsman's power, the mean of several stroke measures is taken.

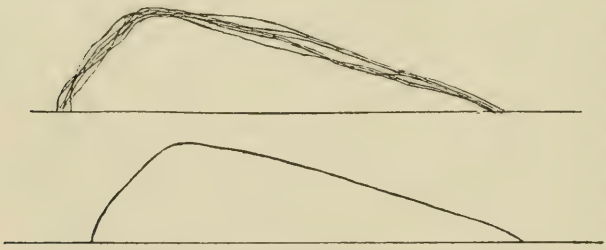

Fig. 3. Deduction of Characteristic Diagram (actual size).

Having now described the indicator, the diagram, and the method of treating it, some account will be given of the results obtained from twenty-seven experiments involving some 2000 strokes of rowing. The principal oarsmen experimented on are denoted by *A*, *B*, *C*, *D*, *E*, *F*, *G*, *H*, *I*.

Fig. 6 represents a series of characteristic diagrams of rowing on sliding seats. *K* was obtained with the earlier indicator, but is introduced here as the stroke-form differs from any others in no small degree. This stroke although very short is nevertheless very powerful.

The horizontal dotted line corresponds to a pull of 100 pounds and the vertical one to a stroke length of 80°.

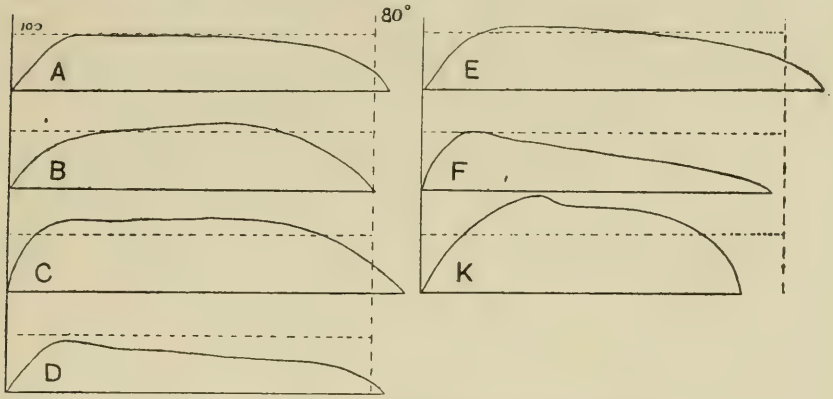

Fig. 4. Some Characteristics—Sliding Seats.

Four fixed-seat characteristics are shown in Fig. 5. The results given in the table are generally deduced from a larger number of measures than the diagrams, and in some cases additional experiments have been introduced, so that the correspondence between the figures and table is not necessarily exact.

An inspection of the characteristics reveals the great individuality in stroke-form that exists even among men who have rowed together and undergone the same course of instruction, and using the same boat and oar. Two diagrams could scarcely be more different than *B* and *F*. The author has found that the form *B* is rather typical of the heavy man's stroke—a powerful stroke, but with a sluggish beginning, while *F* typifies the light man who has a smart beginning, and quickly reaches his highest pressure, which he lacks strength to continue through the stroke.

A comparison of the sliding seat with fixed seat strokes, shows that the latter generally have a much weaker finish, suggesting that the chance of good leg-work at the finish is diminished on a fixed

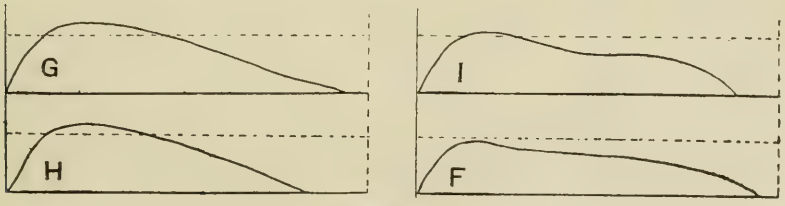

Fig. 5.

seat. As is to be expected, the strokes are some 8 inches shorter. This shows—as an oarsman well knows—that a 14″ slide does not add its full length to the stroke, since the 'swing' in sliding seat rowing is rather shorter than on a fixed seat.

In the table, results are shown of the experiments relating to work done in a gig pair, both on sliding and fixed seats. The author regrets that the stroke rate was not taken in more cases, it having been measured in only seven of the experiments. It is, however, not far from the truth to assume that the estimated rates in other cases are not in error more than one stroke a minute.[1]

The trials involved in the table have all been short, ranging from one to ten minutes, and speaking generally a similar rate of working, with a gradual fall in power, could have been continued for some twenty minutes, while, at high pressure, exhaustion would ensue under similar conditions in less than ten minutes.

ROWING IN GIG-PAIR

Average time between easies—3 min.

Average stroke-rate—22.

1	2	3	4	5	6	7
Oarsman	Stroke length in feet.	Ft.-lbs. per str.	Pull.		Mean.	Horse-power.
			Max.	Mean.	Max.	
I. Sliding Seat.						
A	4·9	381	99	77	·78	0·254
B	4·9	418	115	85	·74	266
C	5·2	571	128	110	·86	380
D	4·9	267	90	54	·60	178
E	5·1	399	104	77	·74	272
F	4·6	340	111	73	·66	196
[K	4·2	498	166	119	·72	331]
Mean	4·9	396	108	79	·73	·259
II. Fixed Seat.						
G	4·5	370	127	83	·65	·247
H	4·0	336	128	85	·65	224
I	4·2	343	123	82	·67	227
F	4·5	312	125	70	·56	208
Mean	4·3	340	126	80	·63	·226

The table indicates that Man Power in rowing is about $\frac{1}{4}$ H.P. and varies with individuals and effort between ·2 and ·4 H.P.

[1] With reference to stroke rate, it is worthy of note that the usual custom of counting strokes, in which the number of strokes in the water is taken, makes an error of more than half a stroke, by omitting one swing forward. Thus if strokes are counted for $\frac{1}{4}$ min. the deduced rate is too high by 2 per minute.

The author regrets that up to the present the only experiment in a racing eight was on an Oxford 'Torpid' in its early days of practice. He suspects that during a race of eight to ten minutes' duration the power would vary between ·3 and ·7 H.P.

The figure given in the sixth column of the table is of some importance, since it roughly indicates the form of the stroke. This coefficient, found by dividing the mean pull during the stroke by the greatest pull, gives a measure of the uniformity of the pull. If the usual coaching maxim of "getting the full work on at once and carrying it out to the finish," were literally carried out, the stroke diagram would become a rectangle, and the coefficient unity.

It is, however, probable that the best results would not be obtained with a high coefficient, since the extra work done does not compensate for the additional fatigue, physical and nervous, involved in setting the muscles suddenly at their fullest tension. In addition to this physiological point there is the mechanical one that the efficiency of propulsion is greatest—other things being equal—when the oar is at right angles to the boat, since it is here that the smallest proportion of the work is devoted to generating kinetic energy in the water.

This question of efficiency of propulsion by oars has been made the subject of experiment by the author. To determine this it is necessary to know, in addition to the information afforded by the indicator diagram, the point about which the oar is turning at every part of the stroke. Since the rowlock is moving forwards, and the tip of the blade backwards, some point between these is neither moving backwards nor forwards. This point may be called the Turning Point.[1] A moment's consideration will show that this point changes its position during the stroke, since the blade first encounters 'dead' water, so that the blade tip at first moves slowly, and the turning point is lower down the oar than a moment afterwards when the water has been set in motion.

By attaching a float to different parts of the oar by a string, so that the float was immersed during the stroke, it was possible to estimate fairly accurately the mean position of the turning point, for which a position 36 inches above the tip of the blade was found, i.e., a point only 3 inches above the top of the blade. This shows how comparatively slight is the motion given to the water, a point which will be more clearly brought out below.

In August of last year the author made an attempt to arrive at more exact results on this point by taking a rapid series of photographs of an oar in motion on the same plate, from a point vertically above the boat. Unfortunately, the only time available, about 3.30 P.M. on a cloudy afternoon, was not an ideal one for exposures lasting

[1] It is clearly the projection of the 'Instantaneous Centre Locus' on the oar.

$\frac{1}{350}$ of a second. However, a photograph was taken from which it has been possible to determine some twenty positions, plotting out a whole stroke, with parts of the forward swing on each side (Fig. 6).

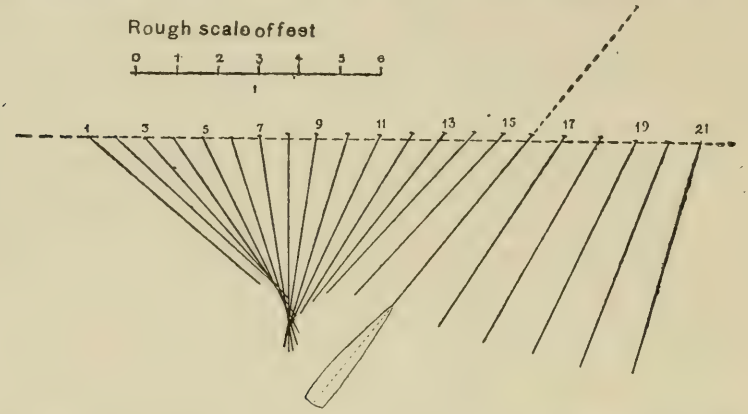

Fig. 6. Motion of an Oar during a Stroke. (Boat is moving towards the right hand.)

The camera was fixed about 23 feet above the River Cam when there was practically no stream to vitiate the results. The exposures were made by a revolving shutter, having three slits, each with an angular width of 5° rotating before a slit of about double that width. The rotation was maintained by such a weight that the speed was fairly uniform during 120 exposures at about 14 exposures per second. As only 26 of the 120 were used, no serious error is introduced by considering the successive photographs taken at equal intervals of time.

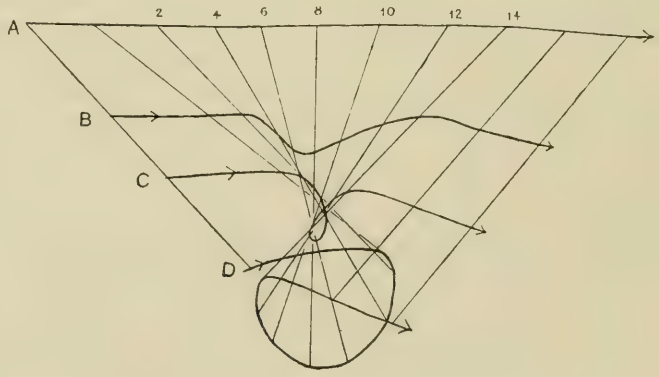

Fig. 7. Curves described by points on the Oars. (*A*), Button; (*C*), 36″ above tip of blade; (*D*), Tip of blade.

Points were marked on the oar at the button, at 36″ above the tip of the blade, and at the tip. The motion of these points is represented by the lines *A*, *C* and *D* in Fig. 7. A projecting part of

the bridge from which the photograph was taken necessitated the camera being tilted slightly out of the vertical, so that there is a corresponding change in the scale of the picture in different parts, but as the important part is the loop of the line *C*, this error is comparatively unimportant.

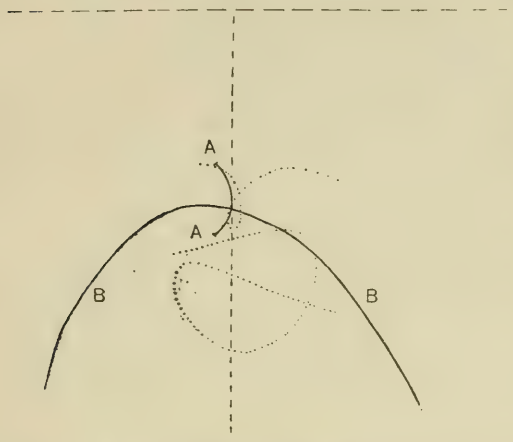

Fig. 8. Motion in space of Instantaneous Centre (*B*) and 'Turning Point' (*A*).

In Fig. 8, *A* represents the locus in space of the turning point determined as the 'envelope' of the various oar positions shown on the photograph, showing that this point moves outwards from the boat in a convex curve facing the bow of the boat.

The curve *B* represents the Instantaneous Centre Locus or point in space about which the oar turns bodily.

Fig. 9 represents the motion of the Turning Point along the oar. From this it will be seen that, starting from a point some 37 inches above the tip, it moves upwards during the first part of the stroke as suggested above, but before the middle of the stroke a curious reaction sets in, and during the rest of the stroke the turning point steadily approaches the tip of the blade, indicating that the blade is coming more and more to rest.[1]

This seems to show that at the beginning of the stroke the blade, which is increasing its distance (see curve *D*, Fig. 7) from the boat's side owing to diminished obliquity, sets up a swirl which moves backwards in the path of the blade, but forwards between that and the boat.

During the second half of the stroke the blade enters this forward moving water and has its motion retarded, taking up thereby some of the energy previously imparted to the water. This point requires further experiment.

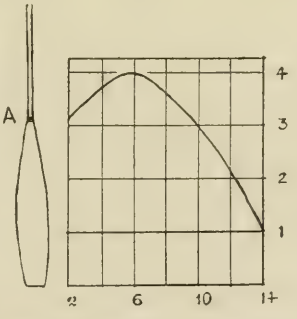

Fig. 9. Position of 'Turning Point' on Oar during the Stroke.

[1] The mean position is found to be 36 inches above the tip, a result coinciding with the rough determination with the float eighteen months previously.

In order to determine the efficiency of propulsion it is only necessary to find what proportion of the whole work done is developed as kinetic energy in the water. If the whole pressure of the blade against the water be P, while the centre of pressure moves through a distance a perpendicular to the blade, the kinetic energy delivered to the water is Pa. For every small angle θ through which the blade turns while the turning point is at a distance x from the centre of pressure, the above product $= Px\theta$.

Without entering into details,[1] the angles between successive positions of the oar, given in Fig. 6, were measured and corrected by interpolation from a curve, while corresponding values of x and P were found from Fig. 9 and C of Fig. 4 respectively.[2]

In this way it was found that almost exactly $\frac{1}{3}$ (33·4 per cent.) of the work was left behind in the water as kinetic energy set up by the oar, giving an efficiency of 66·6 per cent. This efficiency coefficient is concerned, of course, only with the rowing mechanism, and takes no account of physiological waste of energy.

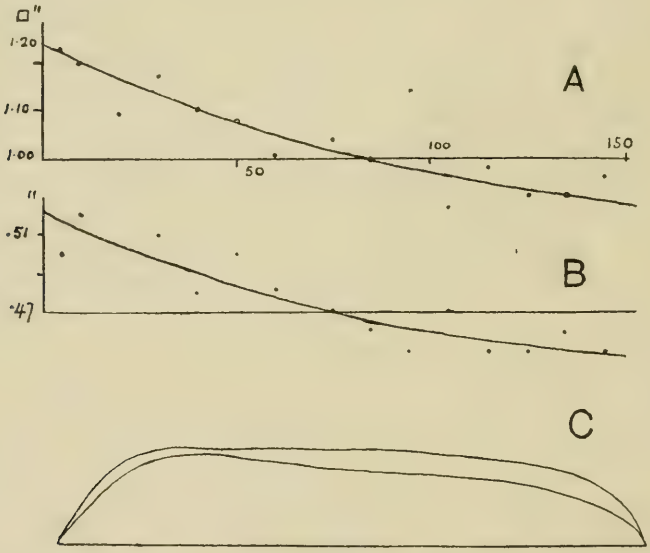

Fig. 10. Fatigue Effect. *A*, Work ; *B*, Greatest Pull ; *C*, Change in Form.

The mechanical efficiency is increased by increasing the size of the blade, so that it can react on a larger body of water, and by increasing the length of the oar, both inboard and outboard, in order to diminish the obliquity at the ends of the stroke. This theoretical possibility is hampered by practical considerations until boatbuilders

[1] See Appendix II.

[2] *C* unfortunately was not the oarsman who was rowing in the oar experiment, but, for a general result, this is of no very great moment.

condescend to turn their attention to making an improvement on present wooden oars and solid outriggers.

In experiments so far described, the only advances made by the new Indicator over the first one have been the greater facility in reducing the results and the possibility of obtaining a mean result from several strokes. Experiments will now be considered for which the continuous record was necessary. These have reference to the effect of fatigue on rowing. Fig. 10 indicates in various ways the growth of fatigue during a continuous piece of rowing. In *A*, ordinates represent the work done during a stroke, whose number from the start is represented by the abscissa. As is to be expected, the strokes vary irregularly, but the steady decrease during the 150 strokes represented is quite clear. *B* represents in the same way the falling off in the maximum pull, and gives a curve almost identical with *A*. *C* shows the change in stroke form during the interval of some 130 strokes, or six minutes' rowing.

It will be noticed that the falling off is most marked during the latter half of the stroke, when the legs and arms take a large share in the work. This suggests that while the powerful system of muscles in the shoulders and back does not easily tire, the legs and arms are comparatively weak.

Fig. 11 represents the change in stroke-form in a four during an interval of 80 strokes. The fatigue-curves in this case are steeper than in the last case, but less regular.

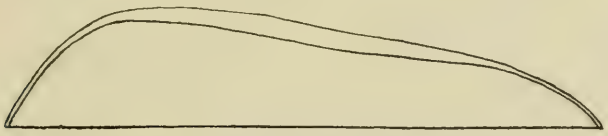

Fig. 11. Fatigue in Four—80 Strokes.

Fig. 12 relates to a journey in a Torpid Eight. This was broken up into about 4 pieces by easies, but again the gradual diminution of power is clear. *C* is an analysis on a larger scale of strokes 210 to 310 during this journey showing the effect of an 'easy.' The diagram shows the maximum pull, and between the two black lines there is clearly a break in which the oarsman has partly recovered his vigour.

With regard to the magnitude of the fatigue effect, in the first case the fall was 18 per cent. in 6 minutes' continuous rowing, 150 strokes; second, 13 per cent. in 100 strokes; third, 22 per cent. in 350 strokes (intermittent). These and other results show that, even in cases where no extreme exertion is called for, fatigue manifests itself, not only as a sensation, but also in diminished

output, and it can easily be imagined how much larger must be the fall in a hard race.

A rough experiment was made to test the use of the indicator for determining the relation between horse power and the resulting velocity of the boat. A pair was rowed between two points, both up and down stream, in order to eliminate the stream velocity. The time and number of strokes was carefully noted. This was

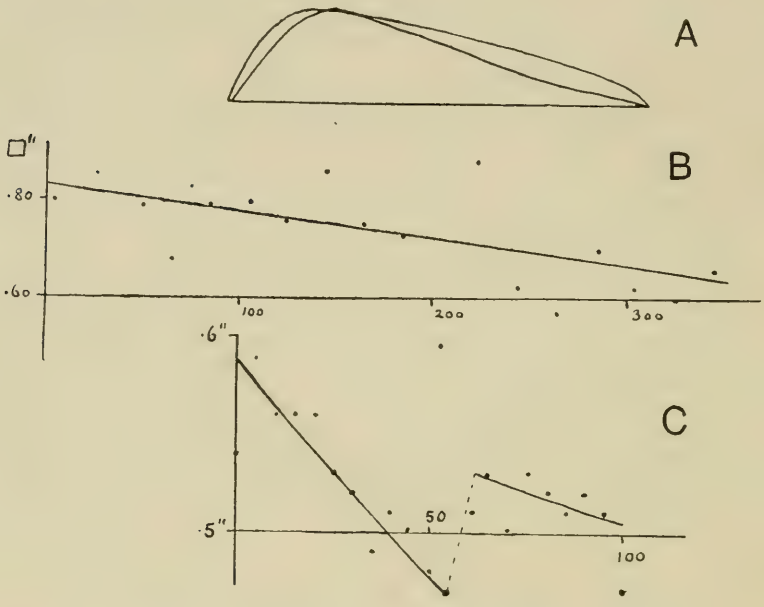

Fig. 12. Fatigue in Torpid Eight. *A* and *B* 350 Strokes, *C* 100 Strokes.

carried out once with very little exertion and again working hard. The measurement of the diagrams, combined with the other observations gave the H.P. corresponding to a certain relative velocity. Assuming H.P. $\propto$ (Vel.)^x, x was found by this experiment to be 2·5—a result probably too low.[1] For the experiment to have a scientific value it would be necessary to indicate both rowers.

The experiment is alluded to as suggesting the possibility of an investigation that might lead to useful results, and although the author has given a description of some experiments which he hopes may be of some interest to the rowing man and also to the physiologist, yet he feels that at every point, more extended, and often more careful experiments, for which he has now no longer the opportunity, would be of interest and importance to science.

E. Cuthbert Atkinson.

Temple Observatory,
Rugby.

[1] See Appendix III.

APPENDIX I

The Oar Used and Constants of Indicator

The axle of the Indicator is 1″ below the button. Midway between the hands is 41·5″ above this point, and the tip of the blade is 102″ below it. The centre of pressure of the blade was calculated on the rough assumption that the pressure at a point varied as the velocity, and it was further assumed that the turning point occupied its mean position 36″ above the tip.

The centre of pressure thus calculated is 9·6″ above the tip.

$$\text{Hence (see Fig. 13)}\ \frac{P}{83}=\frac{F}{184}=\frac{R}{267}.$$

The instrument was calibrated against a spring balance, using a lever with a mechanical advantage of 3·07, and a series of experiments with the spring used throughout, showed 1″ on diagram = 319 lbs. pressure or a pull of 220 lbs.

The radius of the drum is 2″·02, so that 1″ of base line on the diagram represents motion of $F = 1{\cdot}71$ feet.

Finally, 1 square inch of diagram represents $319 \times \frac{184}{267} \times 1{\cdot}71$ or 377 *ft. lbs.*

APPENDIX II

Efficiency of a Stroke in Rowing

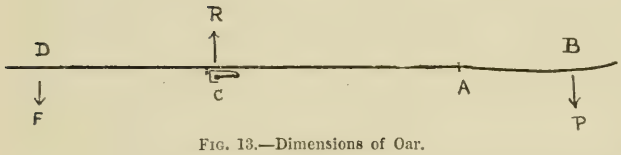

Fig. 13.—Dimensions of Oar.

Let A be 'turning point' while oar turns through an angle $d\theta$, B centre of pressure of the blade, C the button, D centre of pull on handle of the oar.

$$\text{Work delivered to water} = P \times AB\,d\theta = R\tfrac{CD}{BD} \cdot AB\ d\theta.$$

$$\text{Work done by man} = F \cdot CD\ d\theta = R\tfrac{CB}{BD} \cdot CD\ d\theta.$$

So assuming B a fixed point,

$$\text{Efficiency} = 1 - \frac{\int R \cdot AB\ d\theta}{\int R \cdot CB\ d\theta} = 1 - \frac{\Sigma R \cdot AB \cdot \Delta\theta}{\Sigma R \cdot CB \cdot \Delta\theta} \text{ practically.}$$

$\Delta\theta$ between successive positions of the oar was measured for all pairs of positions, and a curve drawn, from which corrected values were deduced.

R was measured in an arbitrary scale for corresponding values of θ on C, Fig. 7, and AB on Fig. .

A table was then constructed as under :—

No.	θ.	$\Delta\theta$.	AB.	R.	$R \cdot AB \cdot \Delta\theta$.	$R\Delta\theta$.
×	×	×	×	×	×	×
7	42·8	8·76	36·6	14·3	4580	125
×	×	×	×	×	×	×

Then by summation :—

$$\Sigma R\Delta\theta = 1049{\cdot}7,\ BC = 92$$
$$\Sigma R \cdot AB \cdot \Delta\theta = 32570$$
$$\therefore E = 1 - \frac{32570}{1049{\cdot}7 \times 92} = {\cdot}666.$$

APPENDIX III

Resistance and Horse Power as a Function of Velocity

In the experiment referred to in the article, the H.P.'s in the two cases were ·294 and ·151 respectively, while the velocities were in ratio 421 to 321.

If H.P. $=k(Vel)^x$ $x=\dfrac{log\ 294-log\ 151}{log\ 421-log\ 321}=2{\cdot}5.$

Comparing this with examples of towing vessels this would appear rather low.

There is, however, a difference between towing, and propelling with internal mechanism.

In former case if Resistance $=kV^x$ where V = velocity,

Horse-power $=kV^x\times V=kV^{x+1}$.

In the latter case, in which the vessel is propelled, for example, with a screw or oar, the force of propulsion is obtained by generating momentum in the water. Making the apparently legitimate assumption that the mass of water encountered by the propeller varies as the velocity of the vessel (probably more correct in the case of the screw than with an oar), since the rate at which the propeller is passing the main body of water is V.

If Resistance $=kV^x$, again

H.P. $=lV^{x+1}+mV^{2x-1}$ where l and m are constants.

So that unless $x=2$, the H.P. required no longer varies as V^{x+1}.

I have nowhere seen this pointed out, and should be glad of any information on the subject.

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II

Scientific Proofs *versus* 'À Priori' Assumptions

UNTIL the great impetus had been given by Darwin to the acceptance of the Doctrine of Evolution, by the publication of his "Origin of Species," natural science mainly consisted of the observation of facts. Thus, old text-books of botany contained the names and descriptions of the various organs of plants with little or no attempt to deal with their physiological uses, much less with their origins. The old idea, that 'species' were fixed entities, created as we now see them, with all their organs complete, led men tacitly to assume that such descriptions were all that was necessary. Botany mostly consisted of the accumulation of morphological facts to aid the systematist. If any suggestions were proposed as to the purpose of this or that organ, the use of which was not very obvious, 'teleological guesses,' as they might be called, were thought to be amply sufficient to account for them. The Bridgewater treatises may be taken as the type of that old method of interpretation of 'uses,' which was, in fact, simply that of *à priori* assumptions without any strictly scientific bases to go upon; by which I mean neither any accurate observations nor experiments, wherewith to verify the supposed uses.

Like that of teleology, it has now come to be generally recognised that the inherent fallacy underlying metaphysics is due to the want of external observations and experimental proofs; so that no worker in natural science can well be a metaphysician at the same time, for the methods of proof—if any such term can be applicable to metaphysics at all—lie in opposite directions. The scientific student should be satisfied only with objective facts; the metaphysician is contented with subjective imaginations.

Darwin published "The Origin of Species" in 1859. This work at once broke down the old ideas of the fixity of species, as having been created such, and having unalterable forms; but the question immediately arose: How are new forms worked out by evolution in nature?

Darwin and Dr Wallace simultaneously propounded the theory of Natural Selection. Though differing in some important points, both based their conclusions on the following statements:—

(1) That more offspring are born than can ever live to maturity and so leave fresh offspring.

(2) That no two individuals of the same kind are ever abso-

lutely alike, in consequence of their 'individual differences'; and these supply material for natural selection to act upon.

(3) That when a being migrates into a new environment, this somehow induces variations to arise in the offspring, which then, it is supposed, vary 'indefinitely,' *i.e.*, in all sorts of directions; but only those best suited to the new surroundings live, all the rest die. Professor Huxley described this process of natural selection of the fittest to survive, as a system of 'trial and error.'

(4) That the rule is that plants of which there is a numerous population are best suited for giving rise to new varieties when some geological catastrophe alters the conditions of their existence, *i.e.*, without migration.

(5) So that those individual plants which possess new variations of structure which render them the best fitted to survive, will do so under those new conditions of existence.

Now the statement No. 1 can be abundantly and easily proved to be true. No. 2 is also quite true. No. 3 is not true so far as varying 'indefinitely' is concerned. This was an *à priori* assumption, which has never been verified, no facts having ever been brought forward to sustain it. No. 4 is also unsupported by any facts; on the contrary, gregarious plants as a rule supply no varieties. No. 5 is a reasonable deduction or *à priori* assumption, had there been any facts to start with. This not being the case the assumption falls to the ground.

On the other hand, observations and experiments prove that all variations which arise in plants are the result directly or indirectly of responses or adaptations to external influences. Such are always 'definite,' to use Darwin's expression, in every case; and whenever this is so, as he himself admits, "a new variety would arise without the aid of Natural Selection." Instead of this being the exception and indefinite variations the rule, as he supposed, the truth is, that definite variation is a natural law admitting of no exceptions at all. Indefinite variations in nature were a pure assumption.

Where, then, is there any opportunity for natural selection to act? It is the universal process in the struggle for life in nature. A neglected lawn, now existing, affords the writer an excellent object lesson. Daisies hold their own over large areas where about four years ago there were none; but among them are sharply defined places in which *Poa annua* or other grasses utterly refuse admission to anything else. Another district is invaded by *Alchemilla arvensis;* yet another consists of large plants of dwarf Dutch clover, while *Achillea millefolium,* with its insidiously creeping stem, constitutes a compact carpet; so does *Galium verum* in many places.[1]

[1] In a lawn made on the side of a heath *Galium saxatile* formed the 'turf' to an almost entire exclusion of everything else.

On the other hand, where grass is allowed to grow to 2 or 3 feet high, all such dwarf plants rapidly disappear. The former conquerors over mown grass cease to be victorious and disappear where their own victims previously had a hard time of it.

Similarly in a neglected kitchen garden a row of peas, unstaked, have succumbed to chicken-weed, *Urtica urens, Solanum nigrum*, groundsel, &c.; but a row supplied with sticks have grown up in defiance of their enemies.

Thus natural selection soon decides what shall be smothered and which shall proclaim itself the fittest to survive. It thus brings about the distribution of plants, but no one has ever shown that its sphere of action in any way concerns the origination of specific characters.

The three principal *à priori* assumptions upon which Darwin's theory was based, then, are as follows:—

(1) The first assumption was that individual variations are the source of varietal variations ("Origin of Species," 6th Ed., p. 34).

(2) The assumption that plants vary indefinitely under changed conditions of life.

(3) The assumption that natural selection eliminates the unfit and retains the fittest to survive among indefinite variations, *i.e.*, of numerous offspring of any one and the same species.

Darwin based his theory mainly upon these assumptions, and more than one of his followers seem to rest satisfied when they say that there is no evidence in the present state of our knowledge, and think that is an all-sufficient answer to the demand for scientific proof. This self-satisfied attitude of professing ignorance of any facts, wherewith to support the theory, seems to me to be the most remarkable, and I would add, deplorable position possible, for any one professing to be a scientific man, to take. I can only regard it as an alarming evidence of the evil growth of a belief in *à priori* assumptions, as if they were scientific methods of proof.

What is the cause of this eager pursuit of subjective will-o'-the-wisps, instead of the study of objective facts? Is it that it is so much easier to sit still and imagine how things may go on in nature than patiently to investigate and accumulate storehouses of facts as Darwin did? Unfortunately he attributed to natural selection a rôle to play which it could not undertake. The result was inevitable. The facts of variation under changed conditions of life are so obvious that his books teem with instances, and then natural selection drops out of sight. When, however, he is arguing on behalf of natural selection, then definite variations seem to be allowed to drop into the background. Hence his "Origin of Species" and "Animals and Plants under Domestication" are full of these two opposite drifts.

We must not forget that the main result of Darwin's writings was to substantiate not natural selection, but the doctrine of evolution. It is this for which our thanks to him cannot be too great; natural selection was but an hypothesis wherewith to account for it, though as we have seen it was based not on facts, but on assumptions, so far as it was supposed to be connected with the origin of species.

Darwin's methods have, unfortunately, led others to follow them. Thus the late Mr Romanes wrote a long and elaborate paper on "Physiological Selection."[1] It was really based on an *à priori* assumption; for he proposed testing his theory or getting others to do it after he wrote and published it. As might be foreseen no subsequent proofs were forthcoming. It still remains what it was at first.

I would venture to beg of writers when advancing a theory, to try and avoid ever using the words 'may be,' 'might be,' 'would be,' or 'must be,' phrases so freely used by Dr Wallace and Dr Weismann. Let them stick to what 'is,' collect innumerable facts in support of any contention, and they will then find little or no thinking or reasoning out will be required at all. For facts soon tell their own tale and quickly make havoc of *à priori*, gratuitous and baseless assumptions.

Indeed, it seems to me that we are rapidly falling back into a position like to, if not the same, as that of the teleologists of the last century. Thus it is frequently asserted by Darwinists that holly has prickly leaves up to a certain height, in order to prevent cattle browsing upon them, and that when the trees grow out of reach, the leaves are non-spinescent. What is this but pure teleology? Only, instead of saying the Creator made them so, as our forefathers would have done, they attribute this supposed result to natural selection. Instead of studying the natural condition of life in which spiny plants grow, they make the above easy but unwarrantable *à priori* assumption, forgetting, or not taking the trouble to observe, whether the holly actually does keep off cattle,—which it does not, for cows are particularly fond of eating holly boughs, as I know to my cost, some well-trimmed bushes having been spoilt by them. Moreover, in early summer when new holly leaves appear, just when they might tempt a cow, they are entirely unprotected, for you may put a bunch of leaves in your mouth and you would not know that they were going to be spiny at all. Lastly, hollies often grow to twelve or more feet high, and retain spiny leaves throughout. So, too, is the same teleological argument held with regard to nettles and their stings, but (alas for the Theory of Protection) numerous species of caterpillars live on

[1] *Journ. Linn. Soc. Zool.*, vol. xix.; 1886.

stinging nettles. Cows often eat them, and man makes 'soup' and 'spinach' of them.

Natural selection, being a purely imaginary agent, is as easy to manipulate as is the Creator's name to account for phenomena, where no proof can be given. As soon as one asks for some grounds for such inferences, the retort comes, "In the present state of our knowledge, it is admitted that there are none!"

It may be now desirable to state what scientific proofs consist of.

There are two lines of evidence possible in support of some deduction arrived at for the interpretation of some natural phenomenon.

The first and best is experimental verification. If you find the result comes as you expected when you have supplied the conditions which, according to your deduction, you supposed to be capable of producing it, then that is all-sufficient and proves your theory to be fact.

Take the case of spinous plants. One first observes as a matter of fact that spiny processes are particularly common in plants growing in arid soils and a dry atmosphere, whereas they do not appear among marsh or aquatic plants. It is always coincidences that one first looks for. Then the question arises, Is the spiny structure in any way due to these external conditions of the environment? Now the test is to grow normally spiny plants in a good soil with plenty of moisture and in a moist atmosphere. Then follows the anticipated result that spines are no longer produced. If they be branch-spines, then the branches grow out into leafy shoots. If they be reduced and spinescent leaves, as in barberry, they at once develop into true leaves. To be quite sure you test it with other plants, and the same result follows. Your theory, therefore, is a proved fact, which henceforth is recognisable as an established natural law.

A different line of proof is required when a deduction cannot be verified by experiment. It must then be established by induction, or the accumulation of probabilities in its favour, until the converse is practically unthinkable. This is the chief line of evidence for establishing evolution as set against the old form of natural theology and teleology.

That I may not lay myself open to the charge of propounding what I have not done myself, I will take my deduction from an observation made in 1870, that irregular flowers are the result of the mechanical action of insects visiting them for honey or pollen. This conception cannot be proved experimentally, as it is impossible to make a regular flower become an irregular one. Another deduction has been drawn by others, namely, that gravity has been the cause of the enlarged lower petal or lip. The

question is, which can bring forward the greater number of correlated facts in support of these two deductions, respectively, *i.e.*, so that they may be supported by inductive evidence? Now gravity is claimed as acting on one petal, but it cannot account for the erect stamens and style in the dead-nettle, and all the rest of the features of the flower. My suggestion is that the irregularity seen in all the organs of the flower are brought about by one and the same cause,—the mechanical action of the insects which visit it.

Assuming the deduction as a working hypothesis, I bring forward (1) abundant evidence to show that protoplasm responds to mechanical forces and builds up structures to meet the strains to which it is subjected; (2) the ribs of the calyx and its form corresponds precisely with the distribution of forces, as seen in Salvias. The form of the corolla is just what would result if it be supposed to be plastic and moulded to the form of an insect, as in *Anhatoda*. The stamens are erect or declinate, in correspondence with a flower having a landing-place on the corolla or not. The honey-gland is situated, and the 'guides' directing to it, precisely in adaptation to the insect visitor. In fact, the entire flower is simply a vast accumulation of innumerable coincidences, all conspiring to one and the same end. It is this which constitutes inductive evidence: while all the correlations are based on the well-known properties of protoplasm, the accumulation of coincidences affords a probability of so high an order as to amount to a 'moral conviction,' and such—all logicians admit—is equivalent to a 'demonstration.'

Similarly, I maintain by inductive evidence that Monocotyledons have descended from aquatic Dicotyledons.

These two lines of proof are amply sufficient, as scientific evidences, to establish the truth of any theory, and convert it into a natural law.

I think I have now said enough to show the utter incompetency of *à priori* assumptions to prove anything at all, of themselves. They are simply deductions, without verifications, and as such remain utterly valueless, and instead of advancing any branch of science, do but retard it, as long as they are accepted without verification. Their danger, however, is subjective not objective. A deduction or *à priori* assumption is useful only as the first step. It must be verified. It is the fatal facility of guessing inherent in mere thinking, irrespective of facts—to collect which is a laborious process, in which Darwin set so grand an example, and upon which evolution was based. This is the imperishable result of his labour, while the theory of Natural Selection, as having anything to do with the Origin of Species, was a quite subordinate matter, and has turned out to be a broken reed to rely upon.

GEORGE HENSLOW.

DRAYTON HOUSE, EALING, W.

III

Natural Gas in Sussex [1]

IN introducing my subject, I do not think it necessary to make any elaborate references to instances of discoveries of Natural Gas in England and abroad. Suffice it to say, that manifestations of natural inflammable gas have occurred in almost every country and geological formation throughout the world, and have frequently been put to practical use.

I will, however, mention what seems to have been one instance of its appearance in London, quaintly recorded by one, Mathew Paris, about the year 1256. Under the head of 'A Sudden Subterranean Explosion,' the chronicler says, "About this time, as some workmen were digging out the bed of an aqueduct in London, to clear the bed of mud (for the water had ceased to flow) a sudden explosion burst forth from the ground accompanied by a flame similar to the fire of hell, which, in the twinkling of an eye suffocated several of the workmen, killing one of them on the spot, and so burning, maiming and disfiguring others that they were entirely useless to themselves ever afterwards. There were some who said that this explosion occurred as by a miracle, because those men were engaged in servile work at an improper hour in the evening." (It quite sounds as if the Factory Acts had been anticipated in these days.)

This interesting record has a somewhat similar parallel in the County of Sussex, and I may quote it for the benefit of well-sinkers personally, and master well-sinkers who may come within the provisions of the Employers' Liability Acts.

I am indebted for the account to Mr Henry Nicholls of Deal, an owner of property at Hawkhurst in West Sussex. He states that between the years 1836 and 1840 a well was sunk at Hawkhurst to a depth of 98 feet. After passing through a certain amount of heavy sand, a blue clay of a very oily flaky nature was met with, mixed with yellow and red streaky clay. This continued to the bottom of the 98 feet. An artesian boring was then commenced, the workmen working by candle-light. Having bored some 50 feet more, or 148 feet from the surface, the augur struck a rock and fell

[1] A paper read at the Conference of the South-Eastern Union of Scientific Societies, Town Hall, Croydon, June 3, 1898.

into a cavity. An inflammable gas immediately ascended, which got ignited by the lights of the workmen. Two men were immediately killed, and as an eye-witness says, the gas burned slowly up the well till it came near the top, when coming in contact with the outer air, it burst out into a sheet of flame, some 20 feet high. It then slowly burned itself out. The water in the well was useless, and Mr Nicholls had the well filled up.

This seems to have been an instance of an inflammable gas occurring in association with strata containing a rock-oil, the gas itself accumulating in a cavity, or what is called by the Americans a 'pocket.' It serves to show that it is unwise for well-sinkers to use artificial lights at the bottom of a well when boring for water, except perhaps in properly constructed mining-lamps.

Another somewhat interesting occurrence took place near Ticehurst Road, Sussex, about six or seven years ago. There is a certain low-lying field, called the 'Bogs Brook,' close to the Ticehurst Road Station of the South-Eastern Railway. It is a marshy spot, and sometimes large bubbles of inflammable gas continuously rise and break on the surface of the pools. One Sunday in a particularly dry summer when the bog was dried up, some boys were about to enjoy a clandestine smoke of tobacco, when a match thrown down suddenly ignited something believed to be inflammable gas. The boys ran away, and the whole field was soon a mass of flame; the peat of the bog also took fire. I am told that the spot, which is in view of the railway, was visited by thousands of people at the time; it burned for a week or more, when some heavy rains soaked the land and put out the fire.

These subjects, although interesting, have but slight interest as compared with the more important occurrences of inflammable gas coming from artesian borings, with a continuous flow during months and years, and existing under a high degree of pressure. Inflammable gas is mentioned by Mr Henry Willett, F.G.S., in his account of the famous Sub-Wealden boring at Netherfield in 1875, as occurring in the Purbeck Strata, and at a short distance above certain strata in the upper Kimmeridge Clays, recorded to be very rich in petroleum. This seems to be the first record we have of a class of gas which has now again been met with in East Sussex. Of course I do not now speak of gases emanating from petroleum at high temperatures, but of certain gases usually found in a free state in association with petroleum, and perhaps, therefore, owing their origin to some common causes and conditions.

An inflammable gas was met with in a boring made by Messrs Le Grand & Sutcliffe (the celebrated hydraulic engineers) at the Heathfield Hotel, Waldron, Sussex, in rocks at a horizon very little

higher than that at which it was discovered by Mr Willett. The foreman of the works made some experiments in piping off the gas. No water was discovered, so the boring was closed up and no more was thought of it until the same firm of engineers, by order of the London and Brighton Railway Company, again made another boring, about 100 yards to the south, commencing in the railway cutting about 43 feet in depth below the level of the top of the former boring. In this boring, but at a greater relative depth, gas was first noticed. I say first 'noticed,' because it is now certain that gas first began to come into the boring at a higher level, perhaps at the same relative level as in the former boring. The rush of gas became greater as the depth increased, and when tested at the top of the bore-tube with a light by Mr E. Head, the station-master at Heathfield, a column of flame sprang up to the height of about 16 feet, and was with great difficulty extinguished. A certain amount of water was discovered, but not sufficient for the Railway Company's purpose, and the boring was abandoned, nearly all the lining tubes being withdrawn. Notwithstanding the partial blocks due to the falling-in of the sides of the bore-hole and the pressure of a great column of accumulated water in the bore-hole, the gas still continues to flow from the bore-tube in considerable quantity. It has been calculated that the pressure of the gas at its source at the bottom of the tube cannot be less than 135 lbs. to the square inch.

It is perhaps somewhat providential that some obstruction has happened to prevent the enormous loss of gas that would have taken place had the tube been left entirely open during a period of, now nearly two years. The Railway Company have screwed a cap on to the end of the tube, with a small half-inch outlet, from which the gas has been allowed to flow continuously.

With the kind permission of the Railway Company, whose officials are giving every kind assistance and facility, my friend Mr Lewis, C.E., F.S.A., and myself have conducted various interesting experiments with the gas; and permission was obtained from the Company for a demonstration of the gas when used in various burners on the occasion of the visit to Heathfield of the Brighton and Sussex Natural History and Philosophical Society on June 11.

Respecting the origin of the gas, we look in vain to the rock-details of the boring for information. It is true that certain small beds of lignite occurred in the section, but one cannot account for the enormous supply and pressure of gas on any theory that the gas emanates from these beds. A portion of the lignite in one of the beds occurred at the depth of 347 feet (at the junction between the Fairlight Clays and the Purbeck Beds), consisting of blue sandy marl-rock with bands of lignite, and has been analysed by Dr J. T. Hewitt (Professor of Chemistry at the Technical College, East

London) on behalf of the Railway Company. He reported to the Company that the lignite contained :—

Moisture	4·90
Volatile matter	15·55
Fixed carbon	1·74
Ash	77·81
	100·00

This record I take it is about the usual result of the analysis of lignite, and I fear throws very little light on the subject.

The greater probability seems to be that the gas is derived from either the Purbeck Beds or the Kimmeridge Clays by percolation through the comparatively porous strata above. The Purbeck beds are known to contain a certain amount of petroleum and bituminous matter, one bed being particularly rich ; but far richer deposits are in the deeper lying Kimmeridge Clays, immediately above which Mr Henry Willett discovered the gas.

This matter, however, as also the subsidiary one of the association of petroleum, is one which can only be determined satisfactorily by means of a deeper boring. The Heathfield borings are much shallower than the bulk of those in North American gas wells, which not uncommonly exceed 1000 feet. The gas which occurs in so many of these American wells is usually the forerunner of a spring of petroleum, and it is possible that the occurrence of gas in large quantities at Heathfield may indicate that there is a larger supply of oil in the petroleum-bearing strata beneath than has been before known to occur at the same horizon in other places.

Dr Hewitt has also reported to the Railway Company on the gas, which, he states, is composed of three constituents :—

Marsh Gas	91·90
Hydrogen	7·20
Nitrogen	·90
	100·0

The first two of the above gases, it may be remarked, are inflammable, but burn only with a blue non-luminous or comparatively non-luminous flame. Nitrogen is not an inflammable gas. It is clear, therefore, that there is nothing in the analysis which can account for the illuminating power of the natural gas at Heathfield, for it burns with a brilliant yellow flame. The gas when burnt in an ordinary ' batswing ' or ' flat flame ' burner is so luminous that the casual observer would not remark the difference between it and ordinary household gas (although the difference does actually exist). Therefore we must suppose either that some luminous property in the gas did not present itself in the sample taken away by Dr Hewitt, or else that some variation has occurred in the constituents

of the gas (a feature which appears to be not uncommon in the Natural Gas of the United States, though I believe not quite to the same extent). I myself and others have seen the gas burning at many times and at different periods, but this non-luminous phase has not presented itself to me or to anyone with whom I have yet met.

I will therefore confine my remarks to the gas in what, out of respect for Dr Hewitt, I will call its normal or luminous phase.

Under these conditions the gas has been carefully analysed by Mr S. A. Woodhead, B.Sc. (Public Analyst for Sussex and Professor of Chemistry at the Agricultural College, Uckfield), and I here beg to record my thanks to him for the time and trouble he has taken to secure the accuracy of his determinations. He constructed his laboratory on the spot at Heathfield, an unlimited supply of gas being supplied to him direct from the bore-hole by means of tubes, and he has taken care to check his results.

The analysis, which is shortly to be published in the *Quarterly Journal* of the Geological Society, speaking roughly, agrees fairly well with that of Dr Hewitt, so far as the presence of Methane (or Marsh gas) is concerned, but Mr Woodhead's analysis reveals the presence of certain hydro-carbons which may make all the difference in accounting for the undoubted illuminating power of the gas. Other important differences between the analyses are outside of the scope of the present paper.

I may remark in general that the Natural Gas, in common with the American Natural Gas, so frequently discovered in association with petroleum springs, is chiefly remarkable for its great heating power when mixed with a large proportion of air. Its main usefulness may thus be said to lie in the direction of lighting by incandescent burners, fuel in manufacturing-engines, and general household purposes.

Tested by Messrs Thorpe & Tasker's photometer, the illuminating power approximates $9\frac{1}{2}$ standard candles. Mr C. E. Masterman, secretary of the Denayrouze Light Syndicate, has kindly tested the gas (forwarded to him in an india-rubber bag). He says (report 3rd, June 1898), he obtained very fine results, and that with a special burner on the Bandsept principle and a C.X. Welsbach mantle, he has obtained 72 candles with a consumption of $2\frac{1}{2}$ cubic feet of gas, at a pressure of $2\frac{1}{2}$ inches, which thus works out to 29·6 candles per cubic foot. This is 15 to 20 per cent. better than London coal-gas.

In using Bunsen burners for heating purposes, it is found best to use about 8 to 10 parts of air to one of Natural Gas. These gases only reach their maximum heating power when a due amount of air is mixed with them, to insure complete combustion.

The Waldron gases are exceptionally fortunate in containing no

impurities of any kind. The chief and main object of the development of a Natural Gas field, is to be found in the use of the gas for fuel in the generation of motive power for various manufactories. In North America there are at least sixteen States using Natural Gas. In the State of Indiana, U.S.A., alone there exist about three hundred Natural Gas companies. The question of the maintenance of the supply is an all-important one. In the American States laws have been passed with a view to economise the use of the gas, which, as now at Heathfield Station, is allowed to rush or burn to waste. Experience abroad shows that even the most abundant fields of gas are capable of exhaustion. Thus in Indiana in 1889, the average initial rock-pressure of the entire field was 325 lbs. to the square inch; in 1896 it had fallen to 230 lbs. to the square inch. On the other hand, such gas has been used for ages in China for the boiling down of brine for making salt. It seems a pity that the two borings at Heathfield should be allowed to seal themselves up without some effort to test the practical utility of this field of Natural Gas. If the tubes were again cleared and relined with perforated tubing, and the water in the tube was pumped out, the important question of the value of the field, which promises so well, might very soon be arrived at without much expense.

Whatever results may accrue from these lighting and heating properties, or whatever the discovery may point to in a commercial direction, the fact nevertheless remains that the discovery is a subject both interesting and instructive, and, I think, worthy of consideration at this conference.

CHARLES DAWSON.

UCKFIELD, SUSSEX.

IV

'Nuclear Reduction' and the Function of Chromatin

EVERY student of biology who remembers the quickening effect of the "Principles of Biology" on his own youthful thought will have greeted Mr Spencer's announcement that a second edition of this great work is in progress, and welcomed the return of the master to his old pursuits and studies. None the less is it a duty to point out that one section of the instalment which we read with so much interest in the May number of *Natural Science* (vol. xii. p. 307) presents a view of the occurrence of 'nuclear reduction' as being an antecedent of fertilisation, which is inconsistent with clearly ascertained facts, and a physiological explanation that is inapplicable to some of the cases where it takes place.

I

The following remarks should have found their correct place in a paper on "The fundamental Principles of Heredity," published in *Natural Science* for October and November last, but were omitted not to overweight it with details of a somewhat abstruse character, and as lying apart from the main object of the study—the tracing out of the cellular pedigree of the organism, with insistence on the point that 'collateral cellular transmission' was operative in all higher organisms. And I would ask the reader to refer to my previous paper in connection with the present one.

'Nuclear reduction' is an easy process to define. When a nucleus is about to divide, its formed matter resolves itself into a definite number of segments; these split each into two, one of which is destined to either of the daughter nuclei resulting from the division. These segments have received the name of 'chromatomeres' or 'chromosomes'; we shall use the shorter, as the more generally received one, though the longer does not carry with it the numerous hypothetical implications, mostly wrong, of the other. Usually, a nucleus on the approach of division reveals the formation of as many segments as entered into it at its formation; and thus the number of segments remains constant from (cell-) generation to generation in the same species; but at a certain point in the life-cycle the number of segments appearing on division is smaller than at the previous divisions of the cells of the parent-cycle; and this is called 'nuclear reduction.'

Three modes of nuclear reduction have been described:—

Case I.—In Elasmobranchs, some Amphibia, and Mammals; in Flowering Plants, some Archegoniates, and Fucaceae. The nuclear network resolves itself at once into half the previous number of segments; certain modifications occur which do not alter the principle of the matter. Here there is clearly no true 'reduction,' any more than if a man received ten shillings from his father and left five florins to each of his two sons.

Case II.—In *Sagitta* and *Ascaris*, in some Gasteropods, and in Liverworts, a modification of this process has been made out, which was first noted by Boveri and Hertwig in 1890; while its relation to Case I. was dwelt on by me in 1891. Here the reduced number of segments appears in a certain cell; but after the first splitting for the coming division a second splitting of each occurs, so that at the first division of the nucleus each daughter nucleus receives the reduced number of segments ready split for the second division; and at the second division the again new daughter-nuclei receive each its own set. This is merely a displacement in time of the two successive splittings for two successive divisions of the nucleus. Similarly, often when a cell is going to undergo two or more successive divisions, the nucleus undergoes the consecutive divisions before the cytoplasm divides once. The reduction is essentially of the same character as in Case I.

Case III.—The nucleus about to divide reveals a number of tetrads or groups each of four segments; the number of groups is only half the previous number of segments, and consequently the total number of segments is twice the original number: the nucleus divides twice, and at each division the several groups are halved between the resulting nuclei; hence it is clear that at the second division each nucleus contains one segment from each group—*i.e.*, a number equal to the number of groups, and half the original number of segments in previous cells of the cycle.

Concerning the details of Case III. there are many conflicting observations, many inconsistent explanations, many vain hypotheses based on the assumption that this is the typical mode of nuclear reduction to which all others are to be forced to conform by some Procrustean process. But we shall not go into these; the explanations that cover Cases I. and II. will cover Case III. also, even if in the last there be something additional left over; but the explanation of this something can well wait until the facts themselves are better made out.

Where does nuclear reduction occur? In Metazoa, usually at the first of the two cell-divisions that give rise to a brood of four spermatozoa, or to the oosphere and the three polar bodies (abortive oospheres) respectively: that is to say, at the inception of the forma-

tion of the sexual pairing cells. The extension of this by zoologists to all cases was tempting; and the demands of the Weismannism of the '80's made this extension appear imperative: reduction or excretion processes in gametogeny were diligently sought for, and of course found everywhere; and in '91 (pp. 62-3) I enumerated and discussed as many as fifteen which had been accumulated in defiance of morphological homology or physiological equivalence. In the same paper, I discussed the question of nuclear reduction from the then state of our knowledge; and pointed out that in Flowering Plants reduction occurs in the pollen-mother-cell, and that this is the equivalent of the asexual spore-mother-cell of Archegoniate Cryptogams (Ferns, Mosses, &c.). "We must remember that the reduction takes place in the pollen-mother-cells of Flowering Plants, which are themselves homologous with the mother-cells that form tetrads of asexual spores in Archegoniate Cryptogams; hence we may be allowed to conjecture that reduction also takes place in the latter group; and by parity that it is not confined to gametogonia[1] [= the mother-cells of a brood of gametes]."

At the time there was only one case, that of a liverwort, that had been at all fully worked out, but since then, we have learned that in the ovule of Flowering Plants reduction takes place at the first division of the primitive nucleus of the embryo-sac; and that in the Archegoniatae without exception, reduction takes place at the inception of the formation of the tetrads of spores, not at that of spermatozoa and oospheres, the equivalents of the sexual cells of Metazoa. Moreover the spore of Mosses gives rise to the leafy plant, capable of indefinite vegetative growth and propagation; that of the Fern to the Fern-scale, which in Gymnogramme, for instance, is perennial also. Thus nuclear reduction is not a process that finds its explanation in the formation of cells specially adapted for 'sexual' (sit venia verbo[2]) fusion or gamogenesis.

I may be permitted to refer to my recent paper ('97) for the exposition of the existence in Metazoa of a long cycle of colonial cell-divisions, alternating with a short one of protistoid brood-

[1] '91, p. 57-8. The sentence closes thus: "but will be found in all mother-cells destined by multiple fission to give birth to a brood of reproductive cells." Of course the latter part of the conjecture has not held good, but the former part has maintained itself: namely that reduction takes place in Cryptogams at *spore-*, not *gamete*-formation. The anticipation thus formulated by me in 1891 was repeated by Overton in 1893, and its enunciation has been ascribed to him by Strasburger ('94a, p. 291; '94b, p. 825), while later the error has been continued by Wilson ('96, p. 196). I did not think such a question of priority worth noting by itself, but take this opportunity of correcting the mistake.

[2] The word 'sexual' has two distinct meanings; the one relating to the fusion of two cells, &c., into one, the other the differentiation of such pairing-cells into two unlike categories such that cells of the one will only pair with cells of the other. 'Sex,' 'sexual differentiation,' 'sexual processes,' are terms as often used in the one sense as in the other; and we may easily avoid the confusion by describing the former as 'pairing processes,' or 'fusion processes,' and the like, and using the additional adjective 'binary' with 'sex,' 'sexual,' to distinguish the latter meanings of the terms.

divisions producing the sexual-cells. In Metaphyta, there are two such alternating cycles of colonial and protistoid growth, the Moss-Plant or Fern-Scale producing the sexual cells, and the Moss-Urn or Fern-Plant producing the asexual spores. In 1891, I wrote of nuclear reduction:—"We may perhaps regard it as an adaptation to prevent the undue multiplication of chromatomeres in the zygote, and the cells produced therefrom." This view has been elaborated by Strasburger; but it will be better, as we shall see, to explain it in another form than his. As, normally, each nucleus exhibits on its division the same number of segments that it had on its formation; the fertilised egg, oosperm, zygote, or whatever we please to call a cell formed by the fusion of two, on its division will present twice the number that were present in either of its two original constituents. If, then, at each sexual fusion this doubling continued, the number of nuclear segments in each cell would increase indefinitely in geometric progression, which is, of course, out of the question: a reduction must take place somewhere. This necessary reduction takes place at the first resumption of protistoid multiplication. In Metaphyta, where there are two such resumptions, this is obvious; in Metozoa there is only one such resumption, which coincides with the formation of the (protistoid) sex-cells,[1] and it is this mere coincidence that gave rise to the idea that reduction was a preparation for cell-fusion, instead of being the necessary consequence of cell-fusion.

A very curious case is that of *Fucus*, the Bladder-Wrack, which, like an animal, has only one colonial form—the familiar plant, and one protistoid reproduction, that producing the sexual cells; here, as we should anticipate, reduction occurs as in Metazoa at the inception of the latter process. Had this case been worked out before that of the Vascular Cryptogams, it would have afforded great support to the physiological hypothesis.

Again, the little fresh-water Algae, the Conjugatae, have their cells isolated, or at most in simple colonies of filaments, where the cells, placed end to end in a single row, divide each on its own account, so that they are really rather protistoid than comparable with the differentiated colonial cells of higher plants. In these plants nuclear reduction occurs at yet another point of the cycle, namely, at the very first cell-divisions of the zygospore, which is, as we see, the resumption of protistoid cell division after conjugation.

Strasburger's statement of this explanation is somewhat different. He writes: "The morphological cause of the reduction in number of the chromosomes . . . is in my opinion phylogenetic. I look upon these facts as indicating a return to the original

[1] I pass by exceptional cases where the reduction occurs at a very early period in the cells of the ovary.

generation from which, after it had attained sexual differentiation, offspring was developed having a double number of chromosomes . . . it is the re-appearance of the primitive number of chromosomes as it existed in the nuclei of the generation in which sexual differentiation [rather cell-fusion, for whether it be sexual or isogamous makes no difference to the point] first took place." If we are to take literally the phrases that I have spaced, we shall have to assume that two such plants as the onion and the turban-lily have independently developed a pairing process; for the number of the nuclear segments is 8 and 16 in the former, 12 and 24 in the latter; the same would apply to the two forms of the roundworm of the horse, with 2 (4) and 1 (2) segments respectively—which is absurd. Yet so much of the essay is taken up in proving that asexual reproduction is the older mode, not only in primitive organisms, but in individual Orders of higher organisms that one wonders if Strasburger has not really missed the inconceivability of his statement as it stands; and hence I cannot accord to the explanation above-given the full weight of his distinguished authority, as I should wish to do.

Now we have seen that the process of 'nuclear reduction,' despite its name, involves no necessary reduction in the quantity of nuclear matter, but only in the number of the segments into which it is distributed. Hence the process cannot have the physiological function ascribed to it as a 'preparation for gamogenesis'; and, since we have noted its occurrence at the inception of a long series of cell-multiplications, this physiological function would be absolutely useless.

II

A word about the functions of the chromatin or nuclein in nuclear division. The amount of chromatin in a nucleus is constantly changing; very often after a cell is formed the nuclein is much reduced in amount, and with this reduced amount the cell does all its individual life-work. At the approach, however, of cell-division, the nuclein grows, and reaches a maximum at the commencement of the nuclear division that precedes that of the cell as a whole; the nucleus of the daughter-cell repeats the conduct of its parent. Whatever be the function of the chromatin in the 'working' cell, as we may term it, it is evidently less important than its function in the dividing cell. The achromatic substance of the nucleus (linin) forms the basis, as it were, of the nuclear segments, the strands on which the chromatin is imbedded in the form of granules, like the string of a necklace, or better, the braid in beaded passementerie; these granules first split, and then the threads on which

they are strung. An explanation far removed from current theories has forced itself on me—perhaps after all it is the achromatic plasma ('linin') of the nucleus whose fair and equal division is the important matter, the final cause of karyokinesis. But the splitting of a viscid thread is one of the most difficult mechanical feats to accomplish. Suppose, then, that there is a certain polarity about the granules of chromatin, through which, after their division, they tend to recede from their fellows as far as possible; through this they will determine a splitting of the filament on which they are strung. The close of nuclear division sees their task accomplished; and, as we should expect, the chromatic granules having fulfilled this appointed task, now atrophy, and remain in this state till the approach of a new cell-division determines a fresh growth of their substance. According to this view the linin is the transmitter of inherited properties, and the chromatin has a purely mechanical function in karyokinesis. I may venture to predict that this hypothesis will be shortly incorporated into the newest edition of the germ-plasm theories; for it avoids the many difficulties due to the ascription of hereditary constancy to a substance so subject to periodic atrophy and growth as the chromatin of the nucleus.

MARCUS HARTOG.

QUEEN'S COLLEGE, CORK.

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SOME NEW BOOKS

Sir William Flower's Essays

Essays on Museums and other Subjects connected with Natural History. By Sir William Flower, K.C.B., D.C.L., etc. 8vo, xv+394 pp. London: Macmillan & Co., 1898. Price, 12s. net.

In this volume are collected together a number of selected essays upon a variety of subjects of biological interest, extending over a period of years between 1870-1897, and, although all or nearly all have been published previously elsewhere and so rendered accessible to students and others, everyone will welcome this new issue, under one cover, of an important series of essays. Sir William Flower has, in these pleasantly written chapters, clearly aimed at interesting and instructing the general reader as well as the man of science, and his happy knack of putting his facts before his readers in a very clear and simple manner should ensure the volume being widely read. Few people have taken more pains to promote in the public at large a healthy interest in biological science, whether in his writings or in his administration of the national Natural History Museum in Cromwell Road. The essays in this volume are not arranged chronologically but, more conveniently, according to subject, under four main headings.

Under the first heading are a series of essays upon Museums. In some respects this may be regarded as the most valuable portion of the volume. When we think of the extremely important part which museums play, and still more might play, in the advancement and dissemination of scientific knowledge, suggestions as to their proper administration from so distinguished an authority, cannot but carry great weight, and it is to be hoped that the valuable hints which abound in these essays may strike home, and be the means of improving the museums, not only of this country but also abroad. It is only within recent years that museum administrators have awakened to a sense of their responsibilities, and the awakening has even now not been general. In these chapters are many eminently practical suggestions as to the proper aims of museums and the methods which should be adopted. The suggested design of a one-storied building for a national natural history museum is at once simple and practical, though it entails as a *sine quâ non* a liberal allowance of ground space. The author's remarks relating to local museums and school museums should be seriously considered by the authorities at the head of such institutions. Were his advice followed the minor museums would become what so few are now, highly instructive—little educational centres, in fact. Each county museum might, by special attention to the systematic collection of objects of local interest, become of real value to specialists as well as to the casual

visitor. Progress, continuity, and a definite system or scheme of arrangement, should be ensured to every museum. Arrested growth or stagnation is fatal to any such institution.

The second division of the volume is entitled "General Biology," and under it are included eight essays upon a variety of subjects of biological interest. The doctrine of evolution supplies the main text of most of the lectures and addresses, and the theory and facts are dealt with in a simple, straightforward manner which will commend itself to the general reader, although the subject as discussed has now lost the charm of novelty which it possessed at the time when the essays were written. The history and progressive work of the Zoological Society affords one theme, and the account of one of our most successful societies, which has managed in the happiest manner to combine valuable scientific investigation with popular instruction, is very acceptable from the pen of the president of the Society. In some respects the two chapters which complete the biological section may claim to be the most interesting. In them the natural history of the Cetacea is discussed in some detail. This group of Mammalia has always been a favourite one with the author. One would have welcomed a more detailed comparative account of the various methods of pursuit and capture adopted in the different 'fisheries,' but the limits of time imposed on a lecture have not permitted this. These two essays, while giving an excellent résumé of the subject, point also to the serious incompleteness of our knowledge of this most interesting and specialised mammalian group. The habits alone of these animals offer a grand field of investigation to naturalists who have time and means at their disposal, our information relating to them being singularly defective. A vigorous research into the foetal development of some of the apparently more primitive species would be a work of great importance. The new gallery at the Natural History Museum, devoted to the Cetacea, and due to Sir William's own energy, should supply a stimulus to further active research.

Under "Anthropology" we find five essays, three being in the form of presidential addresses. The study of Man is dealt with from a general standpoint, and the history and present position of the science are gone into. The author lays much stress upon the importance of the comparative study of the various races of Man, and it is quite clear that long and laborious research will yet be necessary before we are in any position to lay down an even approximately satisfactory classification of the human species. The investigation is beset with difficulties, and even the terminology will require frequent revision. The classification is discussed on a primary basis of three main groups —the black, yellow, and white races—which have hitherto proved the most reliable wide divisions. Much of importance could be added to the scheme of classification as suggested in 1885, but the main points would be left unaltered. An interesting lecture on pygmy races deals with a number of, for the most part at any rate, very primitive peoples, more or less sharply marked off from the races among which they are situated, and from whom they keep aloof. Pygmy races enjoy a wide geographical distribution, though homologies can be traced in even widely separated groups, particularly amongst those referable to the 'black' primary race-division. The essay on "Fashion in Deformity,"

dealing with the many and varied instances of artificial deformation of the person for purposes of fashion, was published in book form ("Nature Series") in 1881, and has probably been very widely read. The customs of this nature are of even greater interest than might appear from a perusal of this article, since there is much to study in the motives which have led to the curious practice, the initiation and other ceremonies associated with many modes of deformation, the test of endurance for which some of them serve, and so forth. Interesting as is Sir William Flower's account as an introduction to the subject, it is high time that a comprehensive general work were compiled from the large mass of information which has now accumulated.

The volume concludes with short biographical memoirs on four great biologists—Rolleston, Owen, Huxley, and Darwin.

H. Balfour.

Packard's Entomology

A Text-Book of Entomology, including the Anatomy, Physiology, Embryology, and Metamorphoses of Insects; for use in agricultural and technical schools and colleges, as well as by the working entomologist. By Alpheus S. Packard, M.D., Ph.D., Professor of Zoology and Geology, Brown University. Pp. xviii. and 730. 1 plate and 654 figures in text. New York: The Macmillan Co. London: Macmillan & Co., 1898. Price, 18s. net.

The title "Text-Book of Entomology" too often denotes a work comprising a meagre outline of the external morphology of insects, and a bare recognition that they possess some internal organs, followed by well-nigh interminable summaries of the characters of orders and families and catalogues of genera. The 'working entomologist' is too easily tempted to devote his whole attention to those outer structures of insects which enable him to classify their multitudinous species, and to neglect those vital organs whose form and development throw so much light on the most interesting problems of insect-life. Hence the 'text-book' is apt to be as dry as the specimens whereof it treats.

Prof. Packard's previous writings would lead us to expect from him a text-book of a very different kind, and students will not be disappointed with the present work. It is divided into three parts. The first deals with morphology and physiology; a short discussion on the place of insects in the animal kingdom, and their relation to other arthropods, is followed by nearly 200 pages devoted to external and 300 to internal anatomy and functions. The second part, consisting of 80 pages, contains a summary of our knowledge of insect embryology. The concluding 130 pages are occupied with an account of insect metamorphosis followed by some speculations as to the origin of the larval and pupal stages. There is no scheme of the classification of insects. A summary of the orders recognised by Prof. Packard would have been desirable, as there is, necessarily, frequent reference to ordinal names; but these will be understood by most who are likely to use the book, which is by no means an elementary treatise. The author does express the opinion that the Collembola and Thysanura are worthy to be separated from other insects as a distinct sub-class, a view hardly tenable when we consider the close similarity of such a springtail as *Japyx* to the wingless earwigs.

The introductory chapter on the relationship of insects to other arthropods is valuable, but it might have come better at the end of the

volume, when the student would have been in a position to attack the problems raised with fuller knowledge of the facts. Prof. Packard goes with the majority of those zoologists who discussed the arthropods some time ago in the pages of *Natural Science*, considering them a group of multiple origin which "may eventually be dismembered into, at least, three or four branches." In the "provisional genealogical tree," which "may serve to show in a tentative way the relations of the classes," the Trilobita are separated from the Crustacea and placed near the common ancestors of the Merostomata (including *Limulus*) and the Arachnida. Prof. Packard therefore rejects the division of arthropods into branchiates and tracheates, considering that the air-tubes of spiders and mites show no necessary relationship between those animals and the insects. *Peripatus* and *Scolopendrella* appear in the direct line of the ancestry of insects, the myriapod stem branching off below *Scolopendrella*. The Diplopoda and Chilopoda are given as separate classes in the tree, though in the text the author seems inclined to defend the old Myriapoda as a natural class against the views of Kingsley and Pocock. Most zoologists will be surprised to see that the millipedes are placed nearer to the insects than are the centipedes; Prof. Packard still attaches considerable importance to the six-legged larvae of *Julus* and *Pauropus*. There is a short account of the anatomy of *Peripatus;* in the points which separate it from the worms it is surprising to find no mention of its reduced coelome and secondarily-formed body-cavity. The description of *Scolopendrella* is fuller—a valuable summary for English readers of the recent researches of Haase, Grassi, and Schmidt on this very interesting creature with an original figure of its internal anatomy. But the statement that *Scolopendrella* "seems to be, like other archaic types, cosmopolitan in its distribution," is puzzling, since a restricted or discontinuous distribution is certainly characteristic of most archaic forms of life.

A synopsis of the characters of insects generally introduces the section of the book devoted to anatomy. The statement here that the labium is formed of the two *laciniae* of the second maxillae fused together seems a curious slip, and the saw-fly grubs are not mentioned among the larvae with functional abdominal limbs. In the detailed account of the external structure of insects, advantage is taken of the latest researches on the mouth-organs, attention being specially directed to the mandibles and first maxillae in the primitive Lepidoptera. A fuller treatment of the piercing and sucking mouth-organs of the Diptera and Hemiptera would have been desirable; the statement is made that functional mandibles are lacking in the latter order, but Prof. Packard does not say if he agrees with Lowne in regarding the lancets usually identified with those appendages as belonging to the maxillae. The epipharynx and hypopharynx are treated at considerable length—a valuable feature in the work, as these organs usually receive much less attention from entomologists than the paired jaws. The author states that Miall and Denny are mistaken in denying the presence of an epipharynx in the Orthoptera. He believes that the insect head consists of at least six segments, including the primitively pre-oral lobe which carries the eyes and ocelli, and the intercalary or tritocerebral lobe between

the antennae and mandibles. The outer skeleton of the thorax is described in detail, and the account of the structure and modifications of the legs and feet is specially full and suggestive, being accompanied by an excellent summary of the mechanics of insect limbs as used for walking, climbing, and swimming. This is mainly derived from Graber's work, supplemented by later researches. Like most American zoologists, Prof. Packard is inclined to allow much weight to the effect of use, disuse, and mechanical pressure in bringing about modifications in limbs. There is even reference to the strange opinion that the absence of tarsal segments from the front legs of certain digging-beetles is a character acquired by frequent mutilation and now become hereditary. In the account of the wings, there is no comparative view of the venation in various orders of insects. There is, however, a good summary of what is known of wing-development both in the lower and higher (metabolous) insects. Prof. Packard rejects the once fashionable theory that insect-wings are modified tracheal gills, believing that they originated in some purely terrestrial form; but he considers it likely that the folds of skin whence they were derived had originally a breathing-function. He agrees with the orthodox view that the elytra of beetles are modified front wings, rejecting their identification with the tegulae of Hymenoptera as suggested by Meinert and Hoffbauer. Not only the cerci and stylets, but also the paired external genital organs are regarded as true appendages of the abdomen. The section on the outer form of insects concludes with an account of such outgrowths of the skin as tubercles, hairs, spines, and scales, and a chapter on insect coloration, the latter dealing rather with the physical and chemical than with the bionomic aspects of that wide subject.

The portion of the book on the internal anatomy and physiology of insects commences with an account of the muscles, followed by a chapter on the nervous system, in which the author accepts Viallanes' threefold division of the insect brain, copying many of his figures, while a well-selected series of figures from Brandt illustrates stages in the fusion of the ganglia of the ventral chain. The histology of the nervous system is dismissed somewhat briefly. On the other hand the sense organs are fully dealt with, and a clear summary of the conflicting views of various naturalists on the method of insect vision, and the functions of dubious antennal structures is valuable; but there is only a mere mention of the ear in the basal antennal segment, described by Hurst and Child. The comparative anatomy of the digestive tract is full and well illustrated, and is followed by a short account of digestion and secretion. Then the salivary and spinning glands, urinary tubes, wax-glands, repugnatorial scent, and poison-glands are dealt with. The author is not inclined to follow Brauer in attaching importance to the number of urinary tubes as showing the relationships of the insect orders. The accounts of the circulatory, respiratory and reproductive systems are excellent; especial care is given to the minute structure of the breathing-tubes, the comparative morphology of the genital ducts, and the formation of the germ-cells.

The section on embryology is mainly drawn from the work of Korschelt and Heider, and is illustrated by copies of their figures, so that it forms a valuable introduction for English students to the

development of insects within the egg. But it is surprising to find no reference whatever to parthenogenesis; the fact that reproduction by virgin females is common in certain insects is now widely known, and some discussion of the subject might be expected even in a much more elementary work than this. Nor is there any mention (except incidentally on the last page of the book) of the fact—most suggestive in its bearing on the possible origin of degenerate groups—that some insects become sexually mature in the larval stage. It is hard to understand what considerations can have led to such omissions as these.

The concluding part of the book deals with metamorphosis, and contains a short summary of the external forms assumed by larvae and pupae, and of the development of the imago from the earlier stages. The structure of the typical larvae in the great orders is by no means fully described; but there is a more detailed description of those interesting forms which pass through what is called a "hypermetamorphosis." The formation of the adult organs in Lepidoptera and Hymenoptera from the imaginal discs is described in detail, while a special section is devoted to the corresponding phenomena in the Diptera which have been studied better than any other order in this connection. Prof. Packard considers the campodiform larva to be more primitive than the eruciform, and figures a series of beetle grubs which show the transition from the one to the other. In a final chapter the fascinating question of the origin of metamorphosis is discussed; and the author comes to the conclusion that the transformations which now characterise all the higher insects have been acquired since the group obtained the power of flight.

A very valuable feature of the book is the full bibliography appended to each section; by means of this the student will be enabled to follow up any subject on which he desires further information. The figures are numerous, and as a whole good. Though the execution is, in some cases, rough, comparing unfavourably with the beautiful drawings in the Cambridge Natural History, for instance, only a few, such as the larva of *Eristalis* on p. 430, can be considered unworthy of the book. Altogether the work supplies a long felt want, and all serious students of insects should be grateful to Prof. Packard for having given them in a single volume so full a summary of what is known about insect structure and life-history.

GEO. H. CARPENTER.

ESSEX VERTEBRATES

THE MAMMALS, REPTILES, AND FISHES OF ESSEX: a contribution to the Natural History of the County (Essex Field Club Special Memoirs, Vol. III.). By Henry Laver, M.R.C.S., &c. 8vo, pp. 1-138, with 8 full-page illustrations. Chelmsford: E. Durrant & Co.; London: Simpkin, Marshall & Co.

FOR many years the attention of those fortunate naturalists who live in the country and have opportunity to observe nature at home has been largely devoted to birds, as is indeed natural from the readiness with which those creatures are seen and studied, and their own extreme attractiveness. It thus happens that while nearly every county in the British Isles possesses a popular work on its birds, the other vertebrates have been very largely neglected.

This is the more to be regretted, seeing that while the occurrence of birds in particular areas must, in many cases, be a mere matter of chance, the presence of other non-volant vertebrates, depending as it does on quality of soil or water, temperature, dampness or dryness, open country or forest, flatness or hilliness, will often illustrate genuine differences in the natural characters of the areas treated of. Therefore, working up from county faunas, we may hope to see in time a scientific classification of British faunal areas, a work which can only be done satisfactorily when the terrestrial vertebrates have been very much more studied than is now the case.

The book before us deals with the vertebrates, other than birds, of the county of Essex; and we may congratulate the Field Club of the county on the charming little work its vice-president, Mr Laver, has produced under its auspices, and may hope that so attractive a book may influence other Essex naturalists to take up the study of the groups it treats of.

According to the author's present knowledge Essex possesses 38 terrestrial and 10 marine mammals, 4 reptiles, 6 amphibians, and 113 fishes, but he expects this number to be considerably increased so far as the marine mammals and fish are concerned.

More than half the book is devoted to the mammals, and Mr Laver has given us a number of interesting notes on the habits and local distribution of the smaller members of the class, as yet so insufficiently studied from the field-naturalist's point of view. This seems to be the most original part of the work, not depending, as so much of the remainder necessarily does, on 'records,' but on the author's personal observations, and the qualities here shown lead us to hope that we may see further contributions from his pen in this direction.

In his nomenclature Mr Laver has wisely followed Mr Boulenger for the lower vertebrates; but in the mammals, with a certain perversity, he tells us that he has accepted the rather out of date Bell and Southwell for the seals and cetaceans, while he has consulted our greatest authority on those very groups, Sir W. Flower, for the Carnivora, Rodentia, and ungulates, with whose nomenclature Sir William has seldom had need to trouble himself. It is not Mr Laver's fault that his book was written just before Mr Miller's researches caused such a *bouleversement* in the nomenclature of our bats, but we may hope that in any future contributions from him a more modern system of nomenclature may be followed.

A last word of commendation must be said for the printing, get-up, and arrangement of the book; while many of Mr Henry A. Cole's illustrations—notably the "Badger Earth, Epping Forest" (p. 42)—are quite charming. O. T.

Wiedersheim's Anatomy of Vertebrates

Grundriss der Vergleichenden Anatomie der Wirbelthiere. By Dr Robert Wiedersheim. Fourth Revised Edition. Jena: G. Fischer, 1898. 8vo, pp. xxiv, 560. Price, unbound, 14 Marks; bound, 16 Marks.

Both students and teachers of the anatomy of vertebrates will welcome the fourth edition of Prof. Wiedersheim's well-known "Grundriss." Although the present volume is smaller than that

of the preceding edition (1893), it is still somewhat unwieldy for a "Grundriss." The original "Grundriss" (1884) was a small octavo volume of 272 pages, and represented an abridged edition of the "Lehrbuch," but it has now increased so greatly in size as practically to have replaced the "Lehrbuch" altogether, for no new edition of the latter work has appeared since 1886.

One of the special features of the last two editions of the "Grundriss"—a feature which primarily distinguished the "Lehrbuch"—is the extensive and valuable bibliography, occupying more than a hundred pages. This commends itself strongly to the advanced student, and, the titles being classified according to the organs or systems of organs dealt with in the memoirs, reference to previous literature is greatly facilitated. One of the disadvantages of the method, however, arises from the fact that many papers treating of several organs demand quotation under a number of headings, and this in the book under consideration has, probably to economise space, not always been accorded. Thus, while Huxley's well-known paper on *Ceratodus* is quoted, as it should be, under the heading "Freie Gliedmassen," it does not find a place under "Schädel der Fische," although it contains most valuable information upon the skulls of *Ceratodus*, *Cestracion*, and *Notidanus*. Numerous other instances might be given, but as it is not possible to suggest a remedy without adding too much to the bulk of the book (except, perhaps, by some system of cross-references) we must be grateful for having our attention directed to even a few papers which in the ordinary course of work might be overlooked.

The profusion of illustrations which invariably characterises the works of Prof. Wiedersheim, and the subordination of the taxonomic to the physiological classification in the arrangement of the chapters, are features which render the book attractive and interesting even to the beginner. It does not follow, however, that a ready sale will be found for the book among English students, for in scope and bulk the present work is almost identical with Prof. W. N. Parker's second English edition, 1897, founded on the third edition of the "Grundriss."

There is abundant evidence of careful editing in the volume under consideration, but entire freedom from mistakes cannot be admitted. *Caudalvenen* appears for *Cardinalvenen* (p. 368, last line but two), and *Metapterygoid* for *Metapterygium* (fig. 96, p. 106), while in fig. 232, p. 275, the letters A and B are transposed. The *ductus endolymphaticus* (fig. 214, p. 252) appears, as in the earlier editions, on the external instead of the mesial surface of the labyrinth, and the side view of the skull of the greyhound (fig. 88 B, p. 96) exhibits four upper incisor teeth; but such blemishes as these are happily few, and the new figures are, on the whole, very good. W. G. R.

The Histology of Vertebrates

Lehrbuch der vergleichenden mikroskopischen Anatomie der Wirbeltiere. II. Schlund und Darm. By Prof. Dr Med. Albert Oppel. 8vo, pp. viii + 682, 4 plates and 343 text-figures. Jena: G. Fischer. 1897. Price, 20 marks.

The second part of Dr Oppel's laborious task carries out the promise of the first. There is the same wealth of detail culled laboriously from multifarious sources and expanded by original matter, and the same careful citation of authorities. The author begins with an

account of the main features of the macroscopic structure of the alimentary canal in the various groups of vertebrates and then enters upon a detailed account of the oesophagus. This occupies the first 160 pages of the volume, and is perhaps remarkable chiefly on account of the close similarity of the structures to be found in different vertebrates. With the exception of the peculiar elaborations forming the crop of birds, there is not any marked difference to be found in the various orders.

In the treatment of the gut the greater variety of structure demands a fuller sub-division of the matter. The systematic treatment has to be repeated for a whole series of structures such as the epithelium, the musculature, the various forms of glands, the lymph-tissues, blood-vessels, and nerves. This occupies the volume up to page 537, and the remaining portion treats of the development and of special structures such as the caecal diverticula and of all such remains. There are copious indices of matter, authorities, and animals. We welcome a scholarly and careful contribution to anatomy.

Ornithologists at Dresden

Aus der 22. Jahresversammlung der Deutschen Ornithologischen Gesellschaft in Dresden, vom 28-30 Mai 1897, herausgegeben von A. B. Meyer. *Abh. K. Zool. Anthrop. Mus. Dresden*, vii. (2). Berlin: R. Friedländer & Sohn, 1898. 4to, pp. vii. 83, pls. i.-iii.

Dr A. B. Meyer has rendered useful service in editing the papers read at the Dresden meeting of German ornithologists. He is himself responsible for a treatise upon the *Paradiseidae*, extending over some thirty-five pages, in which he refers to several of the recent discoveries in this important family, which has of late yielded so many unexpected novelties to collectors. Dr Sharpe and Mr Hartert have already shown a special knowledge of the subject; but it is satisfactory to find our continental colleagues assisting in elucidating the difficult questions which these birds present to the student. It is also pleasant to learn that the Dresden Museum possesses specimens of a good many Birds of Paradise, including several of the original types. The first appendix to this paper supplies a useful list of recent publications dealing with the *Paradiseidae*. Many naturalists will be surprised to learn that we are now acquainted with the eggs of no less than twelve species of Birds of Paradise.

Of more general interest than the foregoing is the charming sketch of bird-life upon the Nile contributed by Dr Koenig of Bonn. Englishmen have of course long since worked out the general features of the ornithology of Egypt; but while our countrymen continue to supplement our local knowledge—witness Mr Cavendish's recent recognition of *Gyps rueppelli* in the Cairo Zoological Gardens, and the capture of the fourth known example of *Saxicola xanthoprymna* by the same veteran 'Ibis,'—there is plenty of scope for other workers, and Dr Koenig's industry is acceptable. His present paper only covers about thirteen pages, but it is brimful of facts and very pleasant reading. Dr Koenig paid special attention to the species of Hirundinidae to be met with in Egypt, including *Hirundo savignyi*, which he naturally considers to be a local and resident form of the more widely distributed *Hirundo rustica*.

An excellent coloured plate reproduces no fewer than three recently discovered sketches of the lost Dodo, or 'Dronte,' as the

Germans elect to style it. Two of these small sketches exist in the Dresden picture gallery. It should also be remarked, that Von Biedermann gives a list of the known representations of the Dodo, numbering fourteen altogether.

Dr Meyer pens a short note upon remains of *Alca impennis* from Swedish deposits, including a left coracoid bone obtained at Greby in the province of Bohuslän, among fragments of pottery and bones of the ox, pig, and sheep.

Finally, we must draw attention to the essay of Mr Voigt upon the love-notes of the Capercailzie or 'Auerhahn' (*Tetrao urogallus*), and the Blackcock or 'Birkhahn' (*Lyrurus tetrix*), which he has taken the pains to set to musical notation; and to a remarkable variety of the common European Kingfisher (*Alcedo ispida*), which is admirably figured. The curious point about this specimen, which was procured upon the Rhine between Mainz and Worms, is that the plumage of the upper surface is half green and half deep blue, the two colours being equally distributed. H. A. M.

Sponge Spicules

Materials for a Monograph of the Ascons. I. On the Origin and Growth of the Triradiate and Quadriradiate Spicules in the Family Clathrinidae. By E. A. Minchin, M.A., Fellow of Merton College, Oxford. Reprinted from the *Quarterly Journal of Microscopical Science*, n.s. vol. xl., pp. 469-587, pls. xxxviii.-xlii., January 1898.

Mr Minchin is almost a social phenomenon. He was appointed nearly five years ago to an Oxford fellowship, after competitive examination in natural science. The formal theory of such fellowships is that they should fill, so to speak, the yolk-sac of an embryonic scholar in some branch of human knowledge, and provide him with the metabolic material known as gold. These halcyon years the fellow (theoretically) devotes to the pursuit of his particular branch of knowledge; he has the opportunity to develop into a fully equipped investigator, and may be supposed in this process to have issued a considerable body of actual research. At the end of that period, on the aboriginal hypothesis as to the place of fellows in the universe, a patron should present the fellow to a fat country living, where, dwelling in the fear of God and the friendship of the squire, the endowed scholar should continue his studies. In the modern world, however, a fat country living is not the natural reward of successful investigation. For this reason it happens, at least in Oxford, that the research fellow seldom does research. Sometimes he goes to the bar; sometimes to medicine; sometimes to commerce; sometimes he enters with ardour into the boarding-school industry, and becomes a useful man to his college. In any event it is almost an anachronism for him to devote the major part of his endowed time to research. How Mr Minchin can reconcile it with his own interests that he has fulfilled the plain purpose of a fellowship by giving up his time to research we cannot pretend to say, but the first part of the monograph now before us, and a series of earlier memoirs, are ample evidence as to the fact.

The present memoir deals with points incidentally obtained in the course of Mr Minchin's attempt to get materials for a complete monograph of the Ascons. For all sponge histology he found it of importance to carry with him, when seeking for specimens, tubes containing

the necessary reagents, and so to secure preparations killed in an absolutely fresh condition. For study of the spicules he used a one per cent. osmic solution diluted with an equal volume of sea-water. After five or ten minutes in this, the specimens were rinsed in water and placed in picrocarmine, in which they were allowed to remain for one or two hours, and were then transferred to glycerine or to alcohol, according as surface-views or sections were required. For spicules he found this method more useful than nuclear stains, as these, from their acidity, corroded the crystals. Certainly the exactness of the results gained, as shown in the description and Mr Minchin's exquisitely beautiful drawings, is proof of the excellence of the method.

After exact description of the spicules and their mode of formation in a series of types, the author passes to a general review of the nature and condition of the spicule systems. He lays considerable stress on the presence of the spicule sheath between the spicules and their secreting cells, and cannot follow the view that spicules are formed in protoplasmic nodes, and owe part of their structure to resulting tensions. He regards the sheath as the remnant of a vacuole secreted by the cell, in the interior of which the spicule itself appeared as a concretion. The primordial form of spicule he takes to be a simple monaxon: the triradiate type he regards as being formed of three monaxons fused at a point. Probably the original monaxons were arranged as the sides of hexagon figures surrounding the pores. It has been objected to a composite origin of the triradiate spicules, that these behave optically as single crystals; yet Mr Minchin shows that in actual ontogeny they arise from the very early fusion of these. The quadriradiate crystals he supports Haeckel in supposing to be derived from triradiate forms by the addition of a gastral ray.

In the case of a memoir like this, which depends in every way upon the exposition of elaborate details, it is impossible to do justice in a short notice. We can only commend it to our readers as a striking and elaborate piece of work, and to the University of Oxford as a singular result of the activity of that supposed rudimentary organ—the endowment of research by fellowships.

The Life Study of Buchanan White

The Flora of Perthshire. By Francis Buchanan W. White, M.D. Edited, with life of Author, by James W. H. Traill. 8vo, pp. lx+408, portrait and map. Edinburgh: Printed for the Perthshire Society of Natural Science by W. Blackwood & Sons. 1898. Price, 7s. 6d.

This handsome and well-printed volume has a melancholy interest to all lovers of British Botany. Dr Buchanan White died in 1894. How much Perthshire Botany owes to his continued labours in the cause of science can only be gathered from the study of this volume. He founded the local Society. He had a large share in raising the Perth Museum to its present position of supremacy amongst those in county towns. The catalogue of his printed works in this volume contains about 227 headings. Moreover although the editor has been obliged to obtain the assistance of others in some of the larger more critical genera such as *Hieracium* (and curiously enough *Salix*), which were not written up by Dr White, still the work is to all intents and purposes his work, and its details and accuracy are exactly what one would have expected from him.

The book contains a portrait of the author, a map of Perthshire, and Prof. Traill has also contributed a memoir, a list of scientific papers, and an introduction. Obviously Dr White's own introduction (43 pp.) is incomplete. Apparently it has been thought best to publish it, so far as possible, without alteration, though some minor changes have been introduced. Hence when the heading "Geology of Perthshire more especially in its relation to the distribution of the Flora" arouses pleasant anticipations of an account of a sorely neglected part of field-botany by one well qualified to judge, we must not be disappointed to find nothing whatever about the Flora, only geological information and a few notes as to the fertility of the soils.

The subdivisions of the county are based on the river-valleys. Fortunately we have a Highland and a Lowland Isla as well as similar divisions for Perth, Earn, and Forth. Most unfortunately the vicious system of Watsonian vicecounties has not been entirely thrown away and this is the solitary fault to be found with this handsome volume. There is in this Flora, as indeed in most county Floras, nothing to show that Darwin or Drude or Engler or Warming or the numerous tribe of German, Scandinavian, and Russian botanists ever existed. The ideas of distribution in this country have stopped with Mr H. C. Watson. If the distribution alone has to be made clear, one might have thought that Mr C. B. Clarke's paper on Tabulation Sub-areas had proved clearly enough that the only satisfactory plan is to make subdivisions based on degrees of latitude and longitude. If these are drawn small enough, any required degree of accuracy in the range of a species can be obtained, and its distribution on a railway line, in a valley or along a mountain chain stands out clearly. For the study of plant-associations or plant distribution on modern lines, this book affords no help whatever. Fortunately the absurd West Perth, East Perth, and Mid Perth have been disregarded. Dr White has gone so far as to separate the Old Red Sandstone districts from the Pre-Cambrian or "Silurian" (p. 4) and this is the line of division followed between the Highland and Lowland Earn, &c. Presumably, like many others, he was afraid to disturb the faith of the average British botanist in the late Mr H. C. Watson.

The actual treatment is best seen by a typical example.

ORDER XXXIX.—ERICACEAE.

TRIBE I.—ARBUTEAE.

1. ARCTOSTAPHYLOS Adans.

A. Uva-ursi Spreng. (87, 88, 89.)

L. O Earn, O O O

H. O Earn, Perth, Isla, O Breadalbane, Rannoch, Atholl.

HAB. Dry rocky places on the mountains. Local.

ALT. Ascends to 2350 ft. in Rannoch and 2000 in H. Earn and Atholl.

Loch Rannoch (Professor Hope, 1762).

Almost confined to the Highland area; but near Crieff it occurs on conglomerate rocks of the Old Red Sandstone.

L. stands for the Lowland Districts on Old Red Sandstone. H. for the Highland on older rocks. "O" means that the plant does not

occur in the district which comes in this position, *i.e.*, in the above case, the plant is absent in lowland Perth, Isla, and Forth, as well as in Gowrie.

But in some cases there is also a short and much needed account of the less known varieties, *e.g.*, *Trifolium dubium*, Sibth. var. *pygmaeum* Soy.-Will. is described as follows, "Dwarfer; petiole of middle leaflet not longer than the lateral ones." These occasional deviations from the plan are probably the most valuable part of the book, and we wish there had been more of them.

Mr F. J. Hanbury has edited the Hieracia, Mr H. Groves has worked through the Characeae, and Messrs W. Barclay and H. Coates have afforded Prof. Traill much assistance with those genera (*Salix*, *Festuca*, etc.), which were not finished in the MS. Prof. Traill's portion of the work must have been exceedingly difficult; he has however succeeded in presenting Dr Buchanan White's work in the best form possible under the circumstances, and deserves congratulation on the accomplishment. The map (by Bartholomew) is very good, coloured according to altitudes of 500, 1000, 2000, and 3000 feet; but is on far too small a scale for detailed work in the field.

The Ferns of the Earth

Die Farnkräuter der Erde. Beschreibende Darstellung der Geschlechter und wichtigeren Arten der Farnpflanzen mit besonderer Berücksichtigung der exotischen. Von Dr H. Christ (Basel). 8vo, pp. xii+388, with 292 figures. Jena: Fischer, 1897. Price, 12 Marks.

In this work Mr Fischer has added another useful book to his long list of botanical publications. Apart from their general life-history the modern botanist knows next to nothing about ferns, and is often fain to admit his ignorance of the names of the most commonly cultivated species. He knows *Pteris aquilina*, the bracken fern, but is surprised to find that a fern which is used for decorative purposes, along with one or more equally unknown palms, in most of the London dairies, or in smaller editions as an ornament for the dinner table, is congeneric. He will find Dr Christ's book very useful. It is not a complete monograph, for such a work would require several volumes of equal size to the one now before us, and it is therefore not intended primarily for the specialist, but notwithstanding includes descriptions of 1154 species belonging to 99 genera. If further information is required, it can be found in one or other of the systematic works of which the author gives a list at the beginning of his book, or of the floras which follow arranged in geographical sequence. Under the title 'Ferns' Dr Christ includes the so-called true Ferns or Filicinae of Prantl which fall into two groups, namely, the Leptosporangiatae, in which the spore-cases are derived from a single (epidermal) layer of cells, and the smaller or Eusporangiatae where several cell-layers are concerned. The first group contains the Hymenophyllaceae, Polypodiaceae, Davalliaceae, Osmundaceae, Cyatheaceae (tree-ferns) and others, the second the two small families Marattiaceae and Ophioglossaceae. The first part of the book (13 pages) is a brief systematic outline of the characters of the groups and genera. The remainder, forming Part II., is a more exhaustive account of these and of the principal species, including all well-established species, "which are in any way remarkable on account of

structure, biological peculiarities, phylogenetic relations, and wide or peculiar distribution." The descriptions are all in German, and those of individual species are remarkably non-technical and lucid. In each case the native habitat is given. The illustrations, which are numbered to correspond with the species depicted, if not of a very high order, are at any rate good enough to be an efficient help in elucidating the text, and assisting in the determination of the specific name. The book concludes with a good index.

The Reproduction of Plants

Beiträge zur Lehre von der Fortpflanzung der Gewächse. By Prof. Dr M. Möbius. 8vo, pp. viii., 212, with 36 figures in the text. Jena: Fischer, 1897. Price, 4 Marks 50 pf.

Prof. Möbius has here brought together the matter contained in several essays previously published in the *Biologische Centralblatt* (1891, 1892, and 1896), which, with some additional chapters, make up a useful and suggestive discussion of the methods of reproduction in plants, both vegetative and sexual, the conditions on which they depend, and the relations between the two kinds. There are five chapters, namely:—1. Introduction. 2. On the consequences of continued vegetative multiplication of plants. 3. On the conditions that govern the flowering of plants. 4. On the relation between spore and bud formation in the reproduction of plants. 5. On the origin and significance of sexual reproduction in the plant-world. In his introduction the author emphasises the contrast between the life of the individual and the life of the species, as the key to the relation between vegetative and spore reproduction, and indicates how the sexual process may have arisen as a secondary result in the latter case. In the second chapter a number of instances are adduced in support of the contention that senile decay, as a result of continued asexual reproduction, exists "only in the imagination of certain authors and breeders." Many cultivated plants, like the sugar-cane, have been propagated from time immemorial only by the vegetative method; many, in fact, can be multiplied in no other way, and there is no evidence to show that these are weaker or less healthy than the offspring of a sexual process. They have no predisposition to disease. Epidemics are not peculiar to vegetatively propagated plants, but appear even among wild plants, both annuals and perennials. An important feature of the book as a whole is the great number of examples which the author adduces in support of his contentions, rendering it useful to the student quite apart from the intrinsic value of the conclusions based thereon.

The Makers of Andrée's Balloon

Andrée and his Balloon. By Henri Lachambre and Alexis Machuron. 8vo, viii + 306 pp., coloured frontispiece, and 44 full-page illustrations. London: Constable & Co., 1898. Price, 6s.

The main interest of this work arises from its detailed description of Andrée's balloon. Whatever geographical results we may learn from Andrée when he returns in the autumn, as there are still good grounds for hoping that he will, his expedition has already done valuable service by its contributions to aeronautics. Andrée, as an

expert mechanician, has introduced many novelties into ballooning equipment. The senior author of the work is Mr Henri Lachambre, the well-known French balloon-maker, who made Andrée's balloon and went to Spitsbergen in 1896 to superintend its installation and inflation. This work was successfully achieved, but the weather was unfavourable and no start could be attempted that year. Few things show better Andrée's courage and good judgment than his determination not to risk an ascent after the first few days of August. Even if he could then have relied on a strong, steady, south wind to carry him to the pole, and a continuation of it on the other side to blow him on either in Siberia or North America, he would have landed too late to make adequate preparations for the winter. Andrée accordingly faced a certain amount of scoffing by returning to Europe in 1896, ready for an earlier start in the following season. Mr Lachambre did not offer to go north again. One summer's exile on the ice-bound shores of Dane's Gat was enough for him. So it fell to the lot of his nephew, Alexis Machuron, to superintend the balloon work in 1897, and to see the actual ascent.

Mr Lachambre's account of his experiences in Spitzbergen is a quaint addition to Spitzbergen literature. He makes numerous observations on the fauna and flora, but his knowledge of natural history is too limited to render these of any value. He says, for example, that the auk is the same bird as the fulmar petrel, and then calls it a duck. As an example of his remarks on the vegetation we may quote the following: "There is no vegetation to gladden our sight, nothing but a few varieties of moss bearing tiny white, violet, and yellow flowers; the yellow ones, larger than the rest, resemble very much the buttercups with which our meadows are dotted in spring." The flower-bearing "mosses" in question are no doubt species of *Ranunculus*, the Arctic poppy (*Papaver medicaule*), and rock roses. The translator by always speaking of eider-geese, for example, helps to caricature the natural history notes in the volume. The photographic illustrations are admirable, and help to atone for the numerous imperfections of the text.

A New Means of Research

Practical Radiography. By A. W. Isenthal and H. Snowden Ward. Second edition. 8vo, 157 pp. London: Published for *The Photogram* by Dawbarn & Ward. 1898. Price, 2s. 6d. net.

We welcome the appearance of this thoroughly practical little book. Now that the popular excitement due to the novelty of Dr Roentgen's discovery is abating, the real workers, those who are earnestly making use of the power placed in their hands, are coming to the front. That good work is being done is very clear from a perusal of the book. This is particularly marked in its application to surgery and medicine; and we are glad to find that one of its first uses has been to the alleviation of human suffering, one great end of scientific research. But there are many other fields where the new power will be of great value. Apart from the physician, men of science interested in Roentgen rays may be divided into two classes: those physicists who are investigating the nature and properties of the rays, and those who are occupied, not so much with the discovery itself, as with its application as a

means of investigation in their own special field of work. Foremost among these are the biologists, and their appreciation of the discovery is evidenced by the recent publication of the beautiful series of radiograms of the British Echinoderms by Prof. R. N. Wolfenden, the British Batrachians and Reptiles by Messrs J. Green and J. H. Gardiner, and many others. We feel sure that the peculiar power of this means of research need only be more fully known to become very largely used.

Returning to the work before us, there are one or two details that we must touch upon. The "Historical Review" is both interesting and comprehensive, but we scarcely think Prof. Roentgen has been given the prominent place due to him; he has certainly done more than to apply to practical uses a series of investigations begun by others; in fact, application to practical uses is just what Prof. Roentgen has not touched upon. We also note the term, 'Crooke's tube'; this is possibly a misprint, but should be Crookes' tube. Also, mention is made of a Rhumkorff (? Ruhmkorff) or Apps' coil; we fail to see the distinction. In chapter ii. (Apparatus), a quantity of very elementary matter is introduced, of which we doubt the utility, for it is not possible, in the space that can be devoted to it, to teach the elements of electricity; the same must be said of the early part of chapter iv. on photography.

The various forms of tubes which have been used for the generation of Roentgen rays are very fully illustrated, and are of considerable interest, as also is the description of the new Tesla oscillator and the more recent form of Ruhmkorff coil introduced by Messrs Rochefort and Wydts; in these particulars the book is thoroughly up to date. The illustrations, particularly the half-tone radiograms, are very good; but it is almost a pity that these latter are scattered about the book without any particular reference to the text. Chapters v., vi., and vii., on medical radiography, show the advance that has been made in this direction within the last two years. The book concludes with a concise and well-written *résumé* of the various theories that have been advanced as to the nature of the rays. On the whole, we can congratulate the authors upon the masterly way in which they have dealt with the subject, and wish the book a wide circulation.

"Science is Measurement"

Notes on Observations, being an Outline of the Methods used for determining the Meaning and Value of Quantitative Observations and Experiments in Physics and Chemistry, and for reducing the Results obtained. By Sydney Lupton, M.A. 8vo, pp. x+126. London: Macmillan & Co. 1898. Price, 3s. 6d.

Although, as indicated in the sub-title, this little book is distinctly intended for the physicist and chemist, still, if we mistake not, it will be of considerable value to the biologist, and, indeed, to students of other branches of natural science. Although there are biologists among us who make much use of mathematical methods in their researches, and although there are mathematicians who love to trespass on the domain of the biologist, yet mathematics are, as a rule, somewhat of a bugbear to the ordinary student of biology, who, it is probable, spent the time he should have devoted to them in running after butterflies or exploring the inside of a frog. For the ordinary biologist

who desires to comprehend something of the work now being done on variation, on selection, on specific differentiae, and so forth, or who may even be bold enough to attempt some of these mathematical methods himself, Mr Lupton's book will come as a godsend. The chapters on Ideas, on Reasoning, on Fallacies, on Laws of Nature, on Cause and Effect, and on Observations and Experiments, with their references to larger works, may be read with profit by many who think they were taught all about such things at school. But it is when we get to the explanation of such things as Units and Dimensions, Averages, Differences, Mensuration, the Use of Tables, Errors, Means, the Law of Error, and Graphical Methods, that the special value of Mr Lupton's book becomes apparent. There are many who will be glad to learn the difference between an Average and a Mean, and more particularly to know which of the various Means is the one now selected by mathematicians as the Mean, while the difference between the Mean and the Mode is another subject upon which light is needed by a good many who are not so well acquainted with Prof. Karl Pearson's writings as they ought to be.

Nevertheless, valuable though Mr Lupton's book is, we should greatly like to see a manual on the same lines, but more especially adapted for the biologist. Nearly all Mr Lupton's are taken, as is natural, from physics or chemistry; Prof. Weldon and his crabs are not so much as alluded to. But the construction of graphical curves to represent biological observations presents difficulties and complications of its own. The useful application of logarithmically ruled section-paper was recently suggested by Mr D. J. Scourfield; while readers who are interested in the determination of species will find an interesting application of mathematical methods suggested by Prof. C. B. Davenport in *Science* for May 20. We merely mention these papers as examples of the kind of thing that might well be incorporated in a text-book of biological mathematics, and we can but regret that so useful a person as Mr Lupton has been lost to our own branch of science.

Historical Specimens of Fossil Cephalopoda

List of the Types and Figured Specimens of Fossil Cephalopoda in the British Museum (Natural History). By G. C. Crick. 8vo, boards, pp. vi + 104. London. 1898. Price, 2s. 6d.

To the serious worker who is called upon to identify organisms nothing is more helpful, and in critical cases essential, than a comparison with the specimen on which the original description was based; and to such persons there is no more welcome aid than a directory of these so-called type-specimens. As regards the British fossils, a Committee of the British Association has long been collecting information with a view to such a list, and owing to its exertions several museums (among others, those of Cambridge, Manchester, and York) have printed and published lists of their type fossils.

The present catalogue enumerates somewhere about 1000 specimens (a striking illustration of the wealth of the British Museum) with full references to the places where they have been described and figured, as well as to their locality and stratigraphical horizon.

They are arranged alphabetically according to the generic names

under which they were first recorded, but there is an index of specific names at the end which renders it easy to find those which have been subsequently placed in other genera. It is not always easy to distinguish with certainty between type-specimens and those which have subsequently been figured; and it was hardly to be expected that any distinction should be drawn between holotypes, cotypes, paratypes, and the like, useful though such distinction is to the systematist. We cannot help thinking that where a heading is repeated, it is a help to the eye to substitute a dash for the word rather than to reprint it each time. There is an error (probably merely clerical) in the account of *Goniatites iris* on p. 57.

Examination Zoology

Text-Book of Zoology. By H. G. Wells, B.Sc., Lond., and A. M. Davies, B.Sc., Lond. University Tutorial Series. 8vo, pp. 366. London: W. B. Clive, 1898. Price, 6s. 6d.

This is a new edition of the text-book prepared some years ago by Mr Wells for the University Correspondence College Press. The original book had many serious disadvantages; the present volume, all of which save one chapter, has been re-written by Mr Davies, is immensely improved. The descriptions are lucid and correct, and the diagrams very useful. We have gone over the volume with close attention, and with no particular favour for the Correspondence College system, but we have confidence in saying that a student who has not the opportunity of attending a regular course of zoology, will gain clear and correct conceptions from this text-book. It will be necessary, of course, for him to go through a considerable practical training in addition, and the authors of the volume constantly impress this upon his notice.

The Metric System

Now that the metric system of weights and measures is legalised in the United Kingdom, everything that tends to encourage its use is to be welcomed. The *Pharmaceutical Journal* has done good service in publishing tables of the metric equivalents of various imperial weights and measures, together with thermometric equivalents in degrees Centigrade, Fahrenheit, and Réaumur. These have now been reprinted as a quarto pamphlet, printed on one side of the paper only, and to be obtained at the *Pharmaceutical Journal* office, 5 Serle Street, Lincoln's Inn, W.C., price 1s. 6d. nett.

Zoology in Japan

All visitors to Japan know the Japanese Cicadidae. There are at least sixteen species found in Japan, of which nine are peculiar to that country. A systematic summary of these species, with the description of a new one, has been published by Mr M. Matsumura in *Annotationes Zoologicae Japonenses* (vol. ii., pp. 1-20, pl. i.). In the same number, Mr A. Iizuka describes a new species of littoral Oligochaete, under the name *Pontodrilus matsushimensis*, since it was discovered in Matsushima Bay, burrowing in the sand under the half-decayed leaves of *Zostera marina*. The species does not agree with Mr Beddard's definition of the genus, in that the *vas deferens* opens

in the same way as it does in the genera *Moniligaster* and *Ilyodrilus*, which belong to other families. Mr Iizuka suggests that this difference is due rather to ignorance of the true structure of *Pontodrilus* than to any divergence in the case of the present species. The last paper, by Mr M. Namiye, describes the indigenous snake *Achalinus spinalis*.

Bibliography

The Geological Survey of Belgium, having with some success published vol. i. of *Bibliographia Geologica*, specifying all works and papers on geological subjects that appeared during 1896, now proposes to issue a retrospective series. The first volume will comprise the titles of all geological publications in the library of the Belgian Survey, arranged according to their subject-matter on the plan of the decimal classification. Authors who wish to see their works quoted in so useful a catalogue will, it is hoped, send copies to the library in question. They may address the Director, Dr M. Mourlon, 2 Rue Latérale, Brussels.

New Serial

We welcome an Italian rival, *Rivista di scienza biologiche*, edited by Dr Enrico Morselli, professor of psychiatry at Genoa University. Subscriptions are received by Dr P. Celesia, 46 Via Assarotti, Genoa.

The Rev. H. N. Hutchinson (Author of "Extinct Monsters") is at present making a large collection of photographs from life of native races of all countries, including Pacific Islands, to illustrate a popular work which he will publish next year in twenty-four sixpenny parts, with Messrs Hutchinson & Co., of 34 Paternoster Row, E.C. For this purpose he requires good, strong, clear photos of men, women and children—some in groups, others as single figures. Silver prints will be preferred. Readers of *Natural Science* who have such photographs are requested to communicate with Mr Hutchinson, through the publishers, with a view to purchase.

Further Literature Received

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OBITUARIES

GEORG BAUR

BORN, JANUARY 4, 1859. DIED, JUNE 25, 1898

THE premature death of Dr Georg Baur, of the University of Chicago, removes one of the most brilliant investigators of vertebrate morphology from the ranks of the present generation of naturalists. He was born nearly forty years ago at Weisswasser, in Bohemia, where his father was at the time Professor of Mathematics. Most of his youth was passed in Hesse and Würtemberg, and his final school was the Gymnasium at Stuttgart. In 1878 he entered the University of Munich, devoting his attention chiefly to zoology, palaeontology, geology, and mineralogy; and in 1880 he removed to Leipzig, where he studied under Credner and Leuckart. Two years later he returned to Munich to take his degree of Ph.D., and from 1882 to 1884 he acted as assistant to Prof. von Kupffer. Dr Baur was then invited by Prof. Marsh to assist him in his work on extinct vertebrata in the Peabody Museum of Yale University at New Haven; and from 1884 onwards he made his home in the United States of America. He visited England in 1886, and again in 1888, and returned to Germany on several short holidays. In 1890 he resigned his assistantship at Yale, and became lecturer in the Clark University at Worcester, Mass. In 1892 he was appointed Assistant Professor of Comparative Osteology and Palaeontology in the newly-founded University of Chicago; and in 1895 he was finally promoted to the rank of Associate Professor, the position he held at the time of his death. He was always intensely active and absorbed in his favourite studies, and the continuous mental strain eventually led to a serious collapse in health last autumn. A few months' rest seemed to revive him, and he returned to Chicago in December; but he was soon compelled to relinquish work again, and the University granted him an extension of leave to visit his relatives in Germany. Paralysis unfortunately supervened, until his mind became completely deranged, and a wide circle of friends has now to mourn his sad and untimely loss.

Dr Baur's researches related chiefly to two subjects—the skeleton of the vertebrate animals, and the natural history of the Galapagos Islands. He was the author of about 150 notes and papers detailing the results of these researches. He also published the first part of a special work entitled *Beiträge zur Morphogenie des Carpus und Tarsus der Vertebraten* (*Jena*, 1888). He was especially interested in the skeleton of reptiles; and although he often arrived at conclusions too hastily, and frequently changed his views, his papers were always filled with suggestiveness and brilliant generalisation, which none interested in the progress of knowledge could afford to overlook. His writings on osteology, indeed, though brief, are among the classics of the subject. Dr Baur's interest in the Galapagos Islands was first aroused by the extinct giant tortoises of North America and the possibility of their close kinship with those of the distant archipelago off the South American coast. It was not until 1891, however, that he

was able to gratify his desire to examine the facts. In this year he visited the islands himself, leaving in May and returning in October, not only making observations but also amassing a most valuable collection of specimens illustrating the fauna and flora of the group. He published his results in a series of important papers, still incomplete at the time of his death. He specially emphasised his discovery that each island had peculiar species of animals and plants, those of neighbouring islands never quite identical though closely related; and he maintained that this harmonic distribution, as he termed it, could only be explained on the hypothesis, that the Galapagos were the summits of mountains of a submerged continental area, which had once been connected with the mainland of America. This heterodox view was at first much opposed, but it has now met with nearly universal acceptance among naturalists. It was approvingly discussed recently by Dr Günther in his presidential address to the Linnean Society; it has also been admitted as plausible by Mr W. Botting Hemsley, the specialist on insular floras. Dr Baur's work was thus far-reaching in the realm of biological and geological philosophy, and an investigator of his originality and broad ideas can ill be spared.

ANTON KERNER RITTER VON MARILAUN

BORN JUNE 13, 1831. DIED JUNE 21, 1898

DR KERNER VON MARILAUN, whose death at the age of 67 is announced, was Professor of Systematic Botany, and Director of the Botanic Gardens and Museum of Vienna University. He is best known to English students from his "Pflanzenleben," perhaps the most charming work on plant-life ever written; it was recently translated into English under the direction of Prof. F. W. Oliver. Another delightful but smaller work is his "Plants and their Unbidden Guests," translated by Dr Ogle, with a preface by Charles Darwin. These are the work of a keen observer and lover of nature, who was gifted with a vivid imagination, which occasionally led him beyond the limit of strict scientific accuracy. Kerner also wrote several books and papers dealing systematically with the flora of Southern Austria, the Tyrol, and neighbouring countries.

FERDINAND COHN, who, on June 25th, died suddenly at Breslau, was born in that town in 1828, educated there, and had held the chair of botany at its University since 1859. Author of several books and papers dealing with plant-life in general, he was best known to the public by his semi-popular work "Die Pflanze," of which the first edition appeared in 1870, the second in 1896. His more strictly scientific work was on the lower groups of plants, such as Algae, Fungi, and Bacteria, and most of it was published in "Beiträge zur Biologie der Pflanzen," an irregular periodical founded by Cohn in 1870 and edited by him till its cessation in 1896. He also edited the "Kryptogamen-flora von Schlesien," which began in 1876 and is still incomplete.

The deaths are also announced of:—Dr PAUL BROCCHI, professor of zoology and director of the National Agronomic Institute at Paris, and president of the Society of Agriculture; and on March 7th, at Columbia, Mo., of E. H. LONSDALE, of the U.S. Geological Survey, and formerly connected with the Geological Surveys of Missouri and Iowa.

NEWS

Among recent appointments, we notice those of : Mr Herbert Bolton, F.R.S.E., who for the last eight years has held the post of assistant keeper in the Manchester Museum, to the curatorship of the Bristol Museum ; Miss Agnes Mary Claypole of Wellesley College, to be assistant in the department of histology and comparative physiology in Cornell University ; Dr Chas. H. Judd, of Wesleyan University, to be professor of physiological and experimental psychology in the School of Pedagogy, New York University, being succeeded as instructor of philosophy at Wesleyan University by Dr Raymond Dodge ; Dr D. S. Miller to be lecturer in psychology at Columbia University, and Dr E. Thorndyke to a similar post at Western Reserve University.

The new buildings of Reading College were opened by the Prince of Wales on June 11th. He explained that the Institution had for its object the advancement of higher education, especially in those branches more particularly connected with the science and art of agriculture. Reading College is an outcome of Oxford University Extension work, and it is hoped that it may be affiliated to the parent University for the purpose of training students in the science and practice of agriculture as part of their University career. For the present, however, Oxford University has rejected this proposal by a very narrow majority.

Lady Warwick has established a small school of science on her estate at Bigods near Dunmow in Essex. Mr E. E. Hennesey of the Royal College of Science has been appointed principal of the school, which is attended by about sixty pupils of both sexes. The Essex County Council is interesting itself in the movement, and we agree with *Nature* that the experiment in every way deserves success.

The people of Birmingham are no longer satisfied with Mason University College, but wish to have a University of their own in which Technical Science should be one of the faculties. A meeting was held in the city on July 4, and speeches were delivered by Mr G. H. Kenrick, who has promised £10,000, Prof. W. A. Tilden, the Bishop of Hereford, and Mr Joseph Chamberlain.

Dr E. D. Pearsons, of Chicago, has presented $25,000 to Pomona College, California, for the erection of a science building.

The Loubat prizes of Columbia University, awarded every fifth year, have this year been given to Mr W. H. Holmes, of the U.S. National Museum (who receives $1,000), and to Dr Franz Boas, of the American Museum of Natural History (who receives $400), for work on archaeology and ethnology of North America. We note that Mr Joseph F. Loubat has recently presented $1,100,000 to the Library of Columbia University.

Prof. J. Reighard, of Michigan University, accompanied by Prof. H. B. Ward, of Nebraska University, Mr A. J. Pieters, and others, is making a biological examination of Lake Erie.

From the report of the Linacre Professor of Comparative Anatomy we learn that the most important recent additions to the objects exhibited in the court of the Oxford University Museum are a cast of the skeleton of the gigantic extinct reptile, *Iguanodon bernissartensis*, from Brussels ; a cast of the skeleton of the extinct theromorphous reptile, *Pareiasaurus baini*, from the Cape Colony ; a cast of the five-toed ancestral ungulate, *Phenacodus primaevus*, from the U. S. America ; a cast of the skull of the gigantic extinct bird *Phororhacos longissimus*, from Patagonia ; and an actual specimen of the extinct shark, *Cladoselache fyleri*, from

near Cleveland, Ohio, which last is intrinsically the most valuable specimen recently added to the collection.

The Museums Association held its annual conference in Sheffield on July 4-8, under the presidency of Alderman Brittain, who, in his address, gave a history of the growth of Museums in Sheffield. The papers read were :—"Museums in Relation to Art Teaching," by Prof. W. C. F. Anderson, of Sheffield, who might have entitled it "Unfortunate experiences of an Art-Student in search of information"; "The Relation of Museums to Elementary Education," by Prof. A. Denny, who dealt chiefly with object-lessons for teachers drawn from zoological subjects; "A History of the Glasgow People's Palace," by James Paton; "Provincial Museums and the Museums Association," by H. Bolton, a suggestion subsequently endorsed by the Council, that a return of all provincial museums in the United Kingdom, their staff, expenditure, and character should be prepared by a committee; "Museums from a Philistine's point of view," by R. E. Ariel Wright; "Note on some Arrangements and Fittings in the Sheffield Museum," by E. Howarth; "Methods of Preservation and Arrangement of Seaweeds for Exhibition," by Prof. F. E. Weiss; "The Arrangement of Museum Herbaria," by E. M. Holmes, who described various methods adopted in various large establishments; "The Advantages of a Gallery of Sculpture (Casts) to an Art Museum," by J. MacLauchlan; "Notes on some Russian Museums," by F. A. Bather, who used certain museums of St Petersburg, Reval, Jurjew (Dorpat), Moscow, Saratow, Astrakhan, Tiflis, Theodosia, as pegs for various controversial subjects; "The Electric Light Installation at the Manchester Museum," by W. E. Hoyle; "The Cleaning of Museums," by Miss Nordlinger, who described the process as it is and as it should be carried out at the Manchester Museum; "The Individuality of Museums," by William White of the Ruskin Museum; "Marine Animals Mounted as Transparencies for Museums," by Dr H. C. Sorby; "The Ethnological Arrangement of Archæological Material," by Harlan I. Smith; and "The Australian Museum," by its secretary, S. Sinclair. During the meeting visits were paid to the Ruskin Museum, the Sheffield Public Museum, the Mappin Art Gallery, University College, the electroplating works of Messrs John Round & Son and Messrs Walker & Hall; and to the works of Messrs Cammell to see the rolling of an armour-plate for H.M.S. 'Ocean.' The meetings of the Association were held in the Council Chamber of the Corporation, and lunch was kindly provided each day by the Lord Mayor. Hospitality was also offered by the President, by Alderman Gamble, and other residents of Sheffield. The meeting concluded with excursions to the Langsett Water Works under the guidance of the engineer, Mr W. Watts, and to Castleton under the direction of Mr John Tym.

Our remarks last month with reference to the salary offered to the curator of the Bristol Museum have led a correspondent to send us the *Western Daily Press* for July 13, from which we are glad to see that there are enlightened councillors in Bristol after all. On the recommendation that the salary of the curator should be £200 *per annum*, Mr Saise asked who assessed the value of the curator's services. He thought that the salary was ridiculously inadequate, and that they would shortly be asked to increase it. Mr Stephens suggested that Mr Saise should have the courage of his convictions and move a larger salary. Personally, he thought £200 sufficient to start with. Mr Lloyd said that the Museum only served a small portion of the city, and, considering that fact, he thought the £2000 per annum which was required for its maintenance was too much. Mr Barker was not surprised at Mr Saise having raised the question as to the salary, because to many people the amount would seem inadequate. But the committee were placed in a difficulty, inasmuch as the salary which Mr Wilson received was £200, and a proposal to increase it would have necessitated the committee coming before the Council, and involved delay in the appointment. The idea

that the Museum was too costly would not be borne out by the facts. It was an exceedingly popular institution, and was undergoing changes which would, he trusted, make it even more popular. They were building up a magnificent reference library, of which the city would be proud in years to come.

The Association of Naturalists of Levallois-Perret has, during the past fourteen years, gathered together the material for a museum, which is open free every Sunday except when the Association is out on an excursion. The collections, says *La Feuille des jeunes Naturalistes*, comprise 5264 species of animals, 4626 of plants, and 4929 geological specimens.

Prof. O. C. Marsh has recently transmitted from New Haven to the Director of the U.S. Geological Survey the fourth large instalment of Vertebrate Fossils secured in the West, in 1882-92, under his direction, as Palaeontologist of the U.S. Geological Survey in charge of Vertebrate Palaeontology. The collection is packed in one hundred boxes, and weighs over thirteen tons. It includes twelve skulls and other remains of the gigantic Ceratopsia from the Cretaceous ; various Dinocerata fossils from the Eocene ; a series of rare specimens of *Brontotherium*, *Elotherium*, *Miohippus*, and other genera, from the Miocene ; a very extensive collection of Rhinoceros and other mammals from the Pliocene ; as well as various interesting fossils from more recent deposits. These will all be deposited in the National Museum of Washington.

The Government has finally stated that it is unable under existing circumstances to embark upon an undertaking of such magnitude as an Antarctic Expedition. What the Government is unable to do the Royal Geographical Society will attempt. Its Council has authorised the president to take steps to obtain subscriptions to the amount of not less than £50,000, while the Society itself will contribute £5000.

Ladies attending the International Congress of Zoologists at Cambridge in the company of a member may become Associates on payment of 10s.

The Zoological Society of London has obtained, through Hagenbeck of Hamburg, a male giraffe, aged one year, from French Senegambia. Though at present only eight feet high he is in good condition, and will, it is hoped, ultimately prove a fitting mate for the female giraffe that has been in the Gardens for some years.

The metric system of weights and measures is to be introduced officially into Russia. The Russians are also considering how they may best abandon their Old Style calendar for that now prevailing in the rest of the civilised world.

At Rueschlikon, on the Lake of Zurich, the shore for a distance of two hundred metres has sunk into the lake, together with some uninhabited buildings standing upon it. The damage done is considerable.

The company established at Lubec, Maine, to extract gold from sea-water, and alluded to in our May number, is said by the *Engineering and Mining Journal* of New York to be "simply another attempt to impose on the credulous."

We notice that a paragraph is going the round of the scientific press, quoting an explanation given in the *Zoologist* by Mr F. R. Godfrey of Melbourne, concerning the sheep-attacking habits of the Kea, *Nestor notabilis* of New Zealand. We do not intend to copy this paragraph into our own pages, because, precisely the same explanation will be found in *Natural Science*, vol. viii., p. 157, March 1896.

The Government of New South Wales has fitted out a deep-sea trawling expedition for experimental fishing off the coasts of the colony. Mr E. R. Waite of the Australian Museum is attached as naturalist, and much valuable material, including many new species, is finding its way to the museum.

Sir E. Braddon, Premier of Tasmania, has issued a proclamation protecting the White-capped Albatross for five years from April last.

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NOTES AND COMMENTS

Vaccination

It is the plain duty of every scientific journal at the present crisis in the history of vaccination, to put before its readers a clear statement of the facts at issue. But it must not be forgotten that two very distinct questions are involved: the efficacy of vaccination is one thing, the expediency of compulsory vaccination is another.

The efficacy of vaccination in preventing small-pox is a scientific fact, established by evidence as clear as any in the range of our knowledge. We do not, nowadays, pause to argue with a man who says the earth is flat. No level-headed person with any capacity for weighing evidence doubts that vaccination, efficiently performed, affords an almost absolute protection against small-pox for a term of five to ten years, according to the natural susceptibility of the individual—that after this period the protection gradually fades, though, in most cases, persisting in some degree throughout life—and that it may be renewed by re-vaccination. Vaccination has been the main agent, in this and other countries, in reducing small-pox mortality to its present low level, and it has done so by abolishing the excessive infant mortality from the disease which prevailed in pre-vaccination times. Statistics show clearly enough that small-pox mortality in any country diminishes strictly in proportion to the extent to which vaccination is carried out. In proportion as re-vaccination is universal, small-pox is reduced to the vanishing point. What mortal man could do in discrediting vaccination was done in the minority report of the recent vaccination commission, an ingenious and plausible piece of special pleading, which was absolutely torn to pieces by Dr M'Vail in his paper read before the Epidemiological Society shortly after.

We take the propositions we have mentioned above as axioms, and we are not concerned to defend them in this place. Their scientific basis rests on the principle that the virus of an infectious disease may be so diminished in effect, by passage through a relatively insusceptible animal, as to produce a mitigated form of the affection, capable, nevertheless, of conferring protection against

the acute disease in its unmitigated form. The recognition of the fact that vaccinia is merely attenuated small-pox—proved now again and again—only brings vaccination into line with what we know of other protective inoculations.

There is, on the other hand, a certain price to be paid for the protection afforded by vaccination. It is the exaggeration of this price which forms the stock-in-trade of anti-vaccinators. The conceivable risks of vaccination are: (1) the constitutional disturbance produced by even an attenuated specific disease in a weakly child; (2) the introduction of some other disease from the vaccinifer in arm-to-arm vaccination; (3) the introduction of pyogenic cocci, normally present in all lymph, and notably in calf-lymph; and (4) the risks attending every scratch on the skin, especially when the recipient of the scratch lives under insanitary conditions. Of the above risks, No. 1 is already largely met by the medical postponement of vaccination in unsuitable cases: it is an infinitesimal risk, but it exists. No. 2, as vaccino-syphilis, is the prop and stay of every anti-vaccinator: authentic cases of this accident are on record, though every medical man knows that the vast majority of supposed cases are in reality examples of congenital syphilis, manifesting itself, as it commonly does, about the time when vaccination has to be performed—the latter serving as a convenient scape-goat for parental sins. This risk is abolished absolutely by the proposed use of calf lymph only. No. 3 is probably an imaginary risk: an exhaustive study of the matter in Germany shows that the course of vaccinia is little, if at all, influenced by the presence of pyococci. In any case the use of glycerinated lymph will abolish what risk exists. There is a strong presumption, based on the observations of Klein, and later on those of Copeman, that the specific virus of small-pox and vaccinia is a spore-bearing bacillus. It has been shown by German observers, and in this country especially by the elaborate researches of Copeman, that, by the admixture of vaccine lymph with diluted glycerine, adventitious organisms are gradually killed off—the more resistant vaccinia virus, presumably in spore form, remaining alone, but retaining its full potency. Such glycerinated lymph may already be obtained in the market, sterile to ordinary bacteriological cultivation, and it is such lymph that the State proposes to offer. Risk No. 4 is untouched by the present bill: it is a question of ordinary cleanliness. Erysipelas may follow an almost microscopic lesion of the epidermis. In practical life we despise such risks, and we can afford to do so.

Even as matters stand under the present laws, the mortality from vaccination is exceedingly minute. Compared with the enormous infant mortality from small-pox in the pre-vaccination era it is infinitesimal. With the introduction of glycerinated calf

lymph the standard objections of the anti-vaccinator are done away with, at least to the unprejudiced mind. The duty of the State is clearly to secure not only the primary vaccination of the largest possible number of infants, but also the re-vaccination of the entire population at the age when the effect of primary vaccination may be presumed to have worn off. This is the verdict of science, and here the functions of science cease.

How all this vaccination may best be secured is a question of practical politics, hardly suited to the columns of a scientific journal. We may state, however, that we can see no logical halting-place between stringent compulsion and the repeal of the Vaccination Acts. For stringent compulsion it may be urged that risk of small-pox is one that affects not so much the man who refuses to have his child vaccinated, as his child and his neighbours, and that in other respects we do not hesitate to coerce the individual for the benefit of the community. But it may be fairly argued, on the other hand, that compulsion has been tried, and has failed; and that the remedy lies in education. The "conscientious objector" conscientiously objects in strict proportion to his ignorance and inability to weigh evidence: he is at the mercy of the irresponsible faddist, who deluges him with fallacious argument and *ex parte* statements. Were the money necessary to secure compulsory vaccination spent in a reasonable system of education of the masses as to the value of vaccination—a matter now entirely neglected—it is possible that in the long run a larger percentage of vaccinations might be secured than under the present unsatisfactory system, or under the illogical makeshift proposed by the present Government.

Christmas Island

Mr C. W. Andrews, of the Geological Department of the British Museum, has returned from Christmas Island (the one south of Java, in about 10° S. and 105° E.) after an absence of fifteen months, ten of which were spent on the island itself. This prolonged stay has enabled him to make fairly complete collections of the flora and fauna, and also to explore and examine the structure of nearly every part of the island. This, Mr Andrews informs us, is very interesting from a geological point of view. It appears to be probably a raised atoll, the central plateau of which is the old lagoon, while the elevations which occur round this are the remains of the islands. Coastwards the land descends in most places by a succession of three or four cliffs, separated by terraces of varying width. In some places these cliffs unite, forming a lofty precipice some 500 feet in height: in these places good sections showing the structure of the island can be seen. At Flying Fish Cove the lower five hundred feet consist of alternations of volcanic rock and foram-

iniferal limestone, above which coral reefs occur. The whole island is thickly covered with forest and jungle, and locomotion is very difficult or even, in places, impossible. The soil is very rich and full of phosphate of lime, beds of which occur on the tops of some of the hills. There are only a few indigenous species of birds and mammals, but these occur in great numbers, rats, particularly, being very unpleasantly numerous.

On the coast immense numbers of frigate birds, tropic birds, and gannets nest in the tall trees. The first named are perhaps the most numerous, and form the chief food-supply of the island. There was an excellent opportunity of observing the remarkable breeding habits of these birds. There are several species of land crabs, including the large *Birgus latro*, which is very numerous over the whole island. These crabs, like the rats, are excellent climbers, and go high up the trees in search of food, and both are a great nuisance to any one camping in the bush. Mr Andrew Ross has lived on the island for some years, and has planted cocoa-nut, bananas, papaias, and other useful plants, and the supply of food is now abundant. Since the climate is very healthy and not too hot, there are many worse places of residence than Christmas Island.

Notes on Sea-Fisheries

The *Report* for 1897 of the Lancashire Sea-Fisheries Laboratory gives every promise of valuable results being obtained in fishery investigations. As pointed out by Professor Herdman, the founding of a laboratory at Piel should enable certain problems, such as those connected with the feeding, breeding and life-history of shellfish, to be taken in hand at once, besides giving opportunities for artificial cultivation, should such be deemed desirable. As regards work upon the food-fishes themselves, the pioneer work has been accomplished elsewhere, and such institutions as that at Piel, beginning where others have left off, should be able to make rapid progress. For example, if the investigations at Piel could be definitely directed to the carrying out of a "scientific experiment which," to quote Professor Herdman, "would gauge the extent of the results of artificial hatching in a given area," they would justify any reasonable expense involved. This investigation might perhaps be carried out in a simpler manner than that suggested by Professor Herdman. Two circumscribed sea-areas, of which the average fish population is to be determined, and ten or more years for experiments are almost unattainable conditions. It is a question how far the importation and cultivation of varieties from another district, the local presence of which at later stages could be readily determined, would solve the difficulty. At least this would be an experiment worthy of adoption.

Another question pressing for scientific solution is the condition of the fisheries relatively to former years. There are many factors which beset this question with difficulty, but there is no doubt that in certain districts, for one reason or another, there has been a diminution of the fish-supply. From Jersey, for example, comes a plain statement by Mr Hornell upon the decadence of the local fisheries (*Nouvelle Chronique de Jersey*, March 1898). This was further borne out by the statements of Mr Renouf at the meeting of the 'Etats' and resulted in the appointment of a special committee to consider the whole subject. In this particular instance the decadence is, according to Mr Hornell, more due to local destruction of young fish than to the more customary British scapegoat, the trawler.

The April number of *La Pisciculture Pratique* contains the presidential address of the 'Conseil supérieur de Pisciculture.' Mr Bellesme has to repeat the old tale of science *versus* politics. His indictment against those in political power is sweeping but perhaps not undeserved. "All their actions are subordinated to the desire of remaining in power and of providing handsomely for their families and for those who keep them at the top of the tree." This is without doubt, telling the truth "franchement et sans circonlocution." The June number contains a very interesting article in connection with the successful introduction of the Californian salmon. At Vicence, the young salmon of 1895 have been reared in a lake near the laboratory and artificial propagation from these is now about to be attempted. Mr Bellesme, in an able paper, points out the method of procedure which should be adopted in order to ensure success. The same writer contributes a general account of the rearing of larval fish.

Steam-Trawling off Jamaica

The Carribean Sea Fisheries Development Syndicate, Limited, is an imposing title. The body bearing this name was subsidised by the Jamaica Legislative Council for 1896 to institute preliminary experiments with a steam trawler in Jamaican waters. Jamaica has to draw very largely upon Canada and the United States for its fish-supply, and it was thought that the adoption of the latest methods of steam trawling in the local waters might be attended with success in the way of profit to those pursuing the industry and of gain to the community of the island. To this end the steam trawler "Capricornus," from the Iceland Fisheries, equipped with well, otter-trawl, and a Grimsby crew was chartered and worked through the district for nearly three months. The record of the log demonstrates pretty clearly that the method of steam trawling is impracticable for this area. The customary ending to each haul appears to have been a torn net, and the actual number of fish caught seems to have been

very small. These results are due, on the one hand, to the luxuriant growth of corals, and on the other hand, to the actual scarcity of the bottom flat-fish or pleuronectids, which with the allied gadoids form the most important elements of our northern fisheries.

The scientific results are of some value. It is to be noted that in many cases in which the lead indicated a sandy bottom, the trawl showed the presence of dense masses of coral, a fact which should be noted by coral-reef theorists. Mr J. E. Duerden, of the Jamaica Museum, gives an able summary of the fauna. *Bergia*, an actino-zoan commensal with a sponge, was re-discovered, and sponges and alcyonarians were found in great abundance. The edible fishes were not abundant, either in actual quantity or in number of species. *Mesoprion*, *Ocyurus*, *Haemulon*, and *Serranus* appear to be the most important.

Apparently the local supply in Jamaican waters will in future, as heretofore, have to depend upon the use of lines and drift-nets.

Ascidians and Bipolarity

We have more than once directed attention to striking cases of distribution, indicating some connection between the North Pacific and North Atlantic. Now comes Professor Herdman (*Trans. Liverpool Biol. Soc.*, xii., pp. 248-267, pls. xi.-xiv., June 1898) and draws up lists of closely-allied species of ascidians from Puget Sound and our own N. Atlantic coasts; similar series are, he says, shown in a subsequent paper on the Crustacea by Mr A. O. Walker. "This, taken along with the similarity between the two faunas shown in other groups, suggests the possibility that there is a common northern circum-polar marine fauna which sends extensions southwards on the western coasts of Europe and America."

Such a conception is of course opposed to the well-known hypothesis of Sir John Murray that the marine faunas towards the poles are genetically more closely related to each other than to any intervening fauna. Sir John has supported this 'bipolar' hypothesis by quotations from the reports of some of the specialists who described the "Challenger" collections. It has struck us that this somewhat crude lumping of the conclusions of many minds, expressed originally with very different ends in view, could lead to no secure result, and we are by no means surprised to find Mr Herdman commenting as follows:—"I do not know how it may be with other authors quoted, but in my case the series of short extracts given from my report require to be expanded and explained, and are then seen not to give Dr Murray's view the support which he supposes. My remarks, on p. 265 of the Report, which he quotes, refer only, it may be stated, to 'Challenger' species. In the genus *Styela*, for example, there are plenty of species known from the tropics. Dr Sluiter has described about fifteen

species from the island of Billiton, between Singapore and Java. I consider that the distribution of Tunicata as a whole does not lend any support to the bipolar hypothesis. On account of the admitted want of equivalence between the characters made use of in specific and generic diagnosis in the different groups mere lists may be deceptive, especially if drawn up and correlated by one man, who cannot possibly be a specialist on all groups of marine invertebrata. For that reason I now abstain from expressing any opinion except in regard to the group of which I have a more intimate knowledge. It seems to me that this matter must be settled by specialists in each group of animals stating their opinions as to the genetic affinities of the northern and southern faunas in their own groups quite apart from and uninfluenced by general lists containing other groups. The Tunicata instanced by Dr Murray, both in his 'Challenger Summary' and in his paper on the 'Marine Fauna of the Kerguelen Region,' help to swell lists that assume rather imposing dimensions, but when I examine the case of these species and genera of Tunicata individually, I find that the records of occurrence have to be added to or modified in such a way as to entirely change the nature of their evidence, and show that there is no such close resemblance between the northern and southern polar faunas as Dr Murray and others have supposed."

Deep Atlantic Holothurians

Another instance of similarity between N. Atlantic and Pacific forms was recently noted by Mr Rémy Perrier in his description of the deep-sea holothurians, Elasipoda, dredged by the "Travailleur" and the "Talisman" (*Comptes rendus Acad. Sci., Paris,* cxxiii., pp. 900-903, Nov. 1896). He describes *Psychropotes buglossa* as being a near ally of *P. varipes* Ludwig, which latter was dredged by the "Albatross" north-west of Cape San Francisco at 1573 fathoms. Unfortunately Mr Perrier does not give either localities or depths of his new species, nor does he figure them ; this is too much to expect in a Preliminary Notice. The chief interest of his paper lies in the rearrangement of the species of *Peniagone* and *Scotoplana* according to the nature of the calcareous spicules, and the establishment on the same grounds of a new genus *Periamna* with triradiate spicules, and having as genotype the *Peniagone naresi* of Théel.

Bipolarity with a Vengeance

We are indebted to Mr Henry Campion of Birmingham for a copy of a 48-page treatise — "The Secret of the Poles" (White & Pike, price 1s.). Mr Campion recognises the impossibility of man ever reaching either pole ; at the same time he appreciates the great importance that such an investigation would have, and does his

best to repair the deficiency by a bold appeal to "the possibilities of creation." The result is "frightfully thrilling." We give a few of the author's statements. "Inside the earth is a hollow region large enough to hide the moon and to spare." "The earth's axis has two openings, one at either Poles (*sic*)." "Metoric swarms and ether are attracted through the axis, as food. One supports the other. The earth does not lose weight but adds to it. Internal combustion." "All winds and tempests originate at and from the Antarctic pole." "The moon has not yet emerged into adult life. She will do so before long," and it will be "a startling epoch in our history." "The heavenly bodies will increase in number, like nebulae, and produce larger bodies." The pamphlet is furnished with a marvellous diagram of the earth in space. We may safely endorse the author's own judgment on his work, that, "whatever its defects, and they are many, it cannot be said it is wanting in novelty, for from the first page to the last the interest is fully kept up."

The Age of the Isthmus of Panama

A reliable account of the geology of Panama has long been one of the greatest of geological *desiderata*, for, in spite of the surveys for railway and canal, and the repeated traverses of the pass of Panama, it has been very difficult to obtain any satisfactory information as to the last date at which marine deposits were laid down upon the summit of the isthmus. Maack's observations have been repeatedly quoted as proving that Pleistocene marine shells occurred on the watershed, and that therefore there was a free waterway across the isthmus in recent times. This view was supported by the zoologists, who, impressed by the general resemblance between the faunas on the two sides of the isthmus, concluded that this could only be explained by a recent direct communication between the two seas. Other workers, however, after a more detailed study of larger collections, have concluded that, in spite of the generic resemblances, the species of the Carribbean and Pacific are almost entirely different, and that therefore the two oceans have been separated for a considerable period. All students of West Indian geology will therefore be very grateful to Professor Alexander Agassiz, who sent Professor R. T. Hill to Panama to settle this question by direct evidence, so that we are no longer dependent on the inferential methods that hitherto have been only available. Professor Hill's report has been issued as one of the *Bulletins* of the Museum of Comparative Zoology (vol. xxviii., No. 5). It is entitled, "The Geological History of the Isthmus of Panama and portions of Costa Rica; based upon a Reconnaissance made for Alexander Agassiz." The report is accompanied by contributions from Dr Dall, Mr T. W. Vaughan, Mr R. M. Bagg, and others, and is illus-

trated by nineteen admirable plates. Professor Hill's work now conclusively demonstrates that there has been no connection across the Isthmus of Panama since the Oligocene period, and that there has not been any very extensive submergence in post-Jurassic times. The connection between the Carribbean and the Pacific must be restricted to a very limited connection in the Eocene and Oligocene epochs. All the interesting theories which explain English glaciation by a Pleistocene submergence of the Panama, and consequent diversion of the Gulf Stream, may therefore be finally dismissed as apocryphal.

Migration and Homotaxis

Geologists know well enough that identity of species in widely separated fossil faunas does not imply identity of age, since time must be allowed for the species to have migrated from one locality to the other. It therefore becomes important to know the actual rate of migration of marine species at the present day. Opportunities of observation are not often presented. One occurred when the Suez Canal put the waters of the Mediterranean into communication with the Red Sea. Another began on Feb. 3, 1825, when the Limfjord Denmark, up till then a fresh-water lake, became connected with the North Sea and its fauna began to change. A study of the immigrant animals was published by J. Collin at Copenhagen in 1884, and a notice of more recent changes has recently been contributed by Th. Mortensen to *Videnskabelige Meddelelser* (1897, pp. 311-319). Since 1884 the following species are known to have entered the Limfjord: *Raia batis*, *Actaeon tornatilis*, *Clavellina lepadiformis*, *Portunus arcuatus*, *Cribrella sanguinolenta*, *Ophioglypha albida*, and *Echinocardium cordatum*. But even yet the fauna is not assimilated to that of the adjoining sea; many echinoderms have yet to find their way in, such as the sand-stars *Amphiura filiformis*, *Ophiopholis aculeata*, and *Ophiothrix fragilis*. We may expect too that some seventy species of molluscs will enter before long, for long ago when the Limfjord was an arm of the sea, and not yet a fresh-water lake, many of these species lived in it and their shells are found along its margin. In the history of the earth a century is but "a watch in the night"; nevertheless the time needed for a species to pass as it were into the next street may suggest to the geologist how long a similar form must have taken to traverse the width of an ocean.

Studies in Auxology

The growth of marine animals is a study attended with difficulty, since thorough observation of individuals is only possible in an aquarium, and there the conditions are inevitably unnatural. Dr C. G. J. Petersen, the director of the Danish Biological Station, has

shown that it is possible to estimate age by tabulating the measurements of a large number of individuals of the same species taken together. The numbers fall into groups, or are concentrated about nodes, each of which would therefore appear to represent the growth of one year. Dr Petersen himself has applied this method to fishes, but it may be used for some invertebrates, as recently shown by Dr Th. Mortensen (*Videnskabelige Meddelelser*, 1897, pp. 319-322). "Some invertebrates" we say, since *Asterias rubens*, *Echinus miliaris*, *Corbula gibba*, *Nucula nitida*, and *Carcinus maenas* resisted all Dr Mortensen's efforts to discover annual groupings. But he was able to show that *Solen pellucidus* reached its full length of 23-28 mm. in two years, while those aged one year had a length of 13-17 mm. Similarly *Ophioglypha texturata* of one year old had discs of 3-4 mm. diameter and arms 8-10 mm. long, while those of two years old had discs of 7-11 mm. and arms of 20-32 mm., and had reached sexual maturity but not yet done growing.

Here we have a field of observation open to any sea-side naturalist in want of work, and open also to collectors of fossils, who might perhaps be able in this way to throw some light on the number of years required for the formation of any given band of fossiliferous rock.

An Evolving Species

Over and over again has it been objected to the theory of evolution of species that no zoologist or botanist has been able to point to the actual origin of a new species. Considering that the world has existed many millions of years, and that men have studied species for scarcely a century and a-half, the objection seems, on the face of it, unreasonable. And yet it has been met more than once. Here is one more instance of what may fairly be described as an evolving species, an instance which has the additional merit of furnishing time-data.

On the north side of Dublin Bay is a tract of sand-hills known as the North Bull, which owes its origin to the alterations caused by the Dublin Harbour works, and certainly has not existed for more than 108 years. This is inhabited now by a numerous race of mice, agreeing in general form and in dimensions with *Mus musculus*, but for the most part of a buff or yellowish-white tint, and differing further from the norm of the species in that they make burrows in the sand and construct nests at the bottom of them. This race has been described in admirable detail by Mr H. Lyster Jameson in the *Journal* of the Linnean Society (*Zoology*, vol. xxvi., pp. 465-473, pl. xxx., 1898). He comes to the conclusion that the colour, which closely resembles that of the sand-hills, is a protective adaptation due to the fact that the short-eared owls and hawks which frequent the North Bull pick out the darker mice,

which, of course, are the ones more readily seen. Isolation also he regards as an important factor in intensifying the effects of competition; "the absence of direct communication with the mainland, and the consequent impossibility of frequent immigrations of dusky specimens from the houses on the adjoining shore, have allowed Natural Selection to carry on its weeding-out of unfavourable variations without disturbances of any kind."

There can be very little doubt that we have here a distinct race that has been evolved during the present century. Mr Jameson gives it no name, not even a sub-specific name, although it is at least as distinct from the species-norm as many named sub-species of *Mus musculus*. We think he is wise, for if the fashion of giving a name to every local race, confined perhaps to a few square miles as here, is to spread, we do not see where bounds are to be set. It is no absurd supposition that there may be many hundred such local races now in existence, each of absolutely independent origin, and yet not to be separated upon internal evidence. The fact is one to emphasise and to remember, but the multiplication of names is no great help.

With regard to the plate we would suggest that an actual rendering of the various colour-tints in separate squares would have been more to the point than this attempt at realism, with its dirty sand and impossible grass.

The Royal Bohemian Museum, Prague

We have frequently referred to the remarkable progress of the Natural History Collections in Prague since they were removed to the new Royal Bohemian Museum. We have now before us the official *Bericht* of the Museum for the year 1897. Notwithstanding limited means and many discouragements, the enthusiasm of the director and his staff continues to overcome all difficulties; and those who know the collection will agree with us when we say that for convenience of arrangement and excellence of labelling it is now one of the foremost in Europe. The cases and fittings naturally lack the elegance and ornamental character of those in the larger and more richly-endowed institutions elsewhere; but so far as the original investigator is concerned, they are all admirably arranged for ready reference, while to the general scientific public they cannot fail to impart such elementary instruction as they desire. When funds fail for the purposes of this arrangement, Dr Anton Fritsch, the distinguished director, himself provides the means, and during the past year he has made a donation of 1000 florins towards the installation of the palaeontological collection. That the Bohemian public appreciate his efforts may be inferred from the circumstance that no less than 78,149 persons visited the museum during 1897;

while the list of original investigators who used the collections for research includes well-known names of several nationalities.

Among the local acquisitions during the year, one of the most remarkable is a hedgehog (*Erinaceus europaeus*), of which Dr Fritsch has published the sketch reproduced in Plate II. In this specimen there are none of the characteristic spines, the whole body being clothed instead with normal hair. Spines, of course, are only highly modified hairs, and this individual is doubtless to be regarded as an example of atavism, one in which the dermal appendages have reverted to their original condition.

Most of the acquisitions are, naturally, Bohemian or are important for comparison with specimens found in the country; for the Museum not only stores collections, but is also the central office for the geological and biological exploration of the kingdom. During 1897 Dr Počta completed the eighth volume of Barrande's well-known "Système Silurien de la Bohême"; Dr Fritsch studied fossil myriapods for his work on the Bohemian gas-coal, and made important geological observations on the Cretaceous rocks; Drs Fritsch and Vávra continued those researches among the organisms of the freshwater lakes of Bohemia, to which we have previously alluded; and the botanists made considerable progress in investigating the local flora, Dr Schiffner paying special attention to the mosses. The Barrande Fund was employed by Dr Perner in continuing his work upon graptolites.

We congratulate our Bohemian colleagues upon their work, and wish them the continued success they so well deserve.

The Geological Survey

Geologists are indebted to the Director-General of the Geological Survey for one of the most interesting and valuable publications of the year. He has decided for the future to issue an annual Summary of Progress, containing not merely the bare blue-book statistics but also a general readable account of the work of the Survey and its bearing upon previous knowledge. He makes a beginning in the Summary for 1897 now before us, which is a well-printed booklet of 176 pages and three index-maps, to be purchased through any bookseller for the small sum of one shilling. We commend it to the notice not only of those interested in our own country, but also to geologists in general who, whatever may be their special studies, are sure to find much of value in it.

This being the first publication of the kind issued by the Geological Survey of the United Kingdom, Sir Archibald Geikie has done well to preface the Summary by an introduction regarding the history and organisation of his department. This introduction occupies 30 pages, and traces the progress of the Survey from its

inception by De la Beche in 1835 to its organisation at the present day. Some technical details as to the staff and field-work during 1897 next follow; and then the new results are summarised in readable form under the respective geological formations, taking them in order from the oldest to the newest. It is difficult to make an adequate abstract of this summary, and it must thus suffice to enumerate a few noteworthy points. Petrologists will find much important new matter in the description of the old rocks of the Scottish Highlands, while stratigraphical geologists will probably turn with greatest interest to the account of the Scottish Silurian formations, in which an entirely new fossil fish-fauna has been discovered. The recognition of Upper Carboniferous strata in the Isle of Arran is also important, although no workable coal-seams have yet been found. In Mesozoic geology there is little to record, but the memoir on the Upper Cretaceous formations is evidently making good progress. In Pleistocene geology there is a wealth of new observations, which students of the glacial period will truly welcome.

New Silurian Fishes

The discovery of Scottish Upper Silurian Fishes mentioned above is briefly reported upon by Dr Traquair, and when fully investigated is likely to prove one of the most important contributions to Biology of recent years. Among these fossils there seems to be clear evidence of a new group of the fish-like organisms now commonly known as Ostracodermi. The new forms are indeed likely to afford important additional information as to the affinities of this problematical group, which has hitherto been best known to us by *Pteraspis*, *Cephalaspis*, and *Pterichthys*. Still more important, however, are nearly complete skeletons of the primitive fore-runners of the sharks. It has long been suspected, on theoretical grounds, that the paired fins of fishes were originally continuous lateral folds of skin supported by parallel bars of cartilage. A few years ago some approach to this condition was observed in the American Lower Carboniferous shark, *Cladoselache*. It now appears, from Dr Traquair's brief notes, that still more important examples of the same arrangement are to be observed in the Upper Silurian genera, exactly where the primitive disposition of parts ought to be found according to theory. Biologists, will anxiously await the completed memoir on these remarkable new organisms.

Extinct Rhinoceroses

The rhinoceroses are typically Old World animals at the present day, and for many years the discoveries of fossil forms seemed to show that they had always been so. It soon became evident from fossils that the rhinoceroses could be gradually traced back both in

Europe and Asia to hornless ancestors in the Miocene period. It was thus clear that they had passed through at least the latest stages of their evolution in these regions. In 1850, however, Professor Leidy first found a fragment of a rhinoceros in the Miocene of North America; and since that time so many remains—including several nearly complete skeletons—have been found in the United States, that these great quadrupeds are now proved to have been at least as abundant in North America as in Europe and Asia, during the Miocene and Pliocene divisions of the Tertiary period. It is true, indeed, that in North America the rhinoceroses never acquired a typical horn, while they became extinct before the close of the Pliocene period; but they attained a truly remarkable development, and we now know more of the characters of the family from the discoveries made in North America than from those in Europe and India.

Professor H. F. Osborn, the well-known Curator of Vertebrate Palaeontology in the American Museum of Natural History, New York, has just begun to summarise our present knowledge of these extinct New World rhinoceroses in the first part of a beautifully illustrated quarto *Memoir* issued by the American Museum (vol. i., pt. 3, 1898, pp. 75-164, pls. xii*a*.-xx., April 22, 1898). It appears that three distinct groups can now be recognised, adapted for different modes of life. Firstly, there were the Upland or Cursorial Rhinoceroses, such as *Hyracodon*, all agile, slenderly-built animals, with three toes, somewhat simulating the three-toed Miocene horses with which they were associated. Secondly, there were the Aquatic Rhinoceroses, such as *Metamynodon*, with great tusks, simulating the modern hippopotamus of Africa. These were short, heavy animals, with four-toed spreading feet, and probably a prehensile lip. Thirdly, there were the True or Lowland Rhinoceroses, very abundant and doubtless similar in habit to their surviving congeners in Asia and Africa at the present day, though, as already remarked, destitute of the characteristic horn. These three groups were differentiated in North America before the close of the Eocene period, and there is already some fragmentary evidence of a similar differentiation in Europe, though the materials as yet available for discussion are too imperfect to be conclusive.

The present instalment of Professor Osborn's Memoir discusses the morphology of the teeth and skull of the Rhinocerotoidea, tracing the gradual divergence of the three types, occasionally obscured by parallelisms and convergences. The true rhinoceroses, Rhinocerotidae, are then discussed from the points of view of habits, geological history, and morphology, while the section ends with a preliminary bibliography that will be of much value to students. Then follows a detailed account of the hornless rhinoceroses, Aceratheres, collected

by the expeditions of the American Museum under Dr J. L. Wortman, during 1892 and 1894, from the Oligocene White River Beds of Nebraska and Dakota. These present a very large and perfect series of skulls, many of them associated with fairly complete skeletons.

Future parts of the monograph will deal with the Aceratheres of the American Miocene; the Aceratheres and Rhinoceroses of Europe in comparison with those of America, skeletal characters of American Aceratheres, and final classification of the Rhinocerotidae; the Amynodontidae; and finally the Hyracodontidae. Professor Osborn states that it is his one single purpose "to establish a sound philosophical basis for the morphology of the Rhinoceroses, derived from their primitive, parallel, and divergent characters, and leading toward the discovery of their origin, phylogeny, and distribution." If the remaining parts of his Memoir are like that now before us, he is fairly assured of success.

On Cyclamen

The well-known genus *Cyclamen* is the subject of an exhaustive memoir by Dr Friedrich Hildebrand of Freiburg, which has been recently published by Fischer of Jena. The thirteen species are almost confined to the Mediterranean region, spreading northwards only as far as southern Germany, and eastwards to the Caucasus. *Cyclamen* is a good example of adaptation to the climatic conditions prevailing in the district in question. A season of luxuriant growth alternates with a season of rest, but the determining factor is not, as in higher latitudes, the appearance or disappearance of continued frosts, but variation in the amount of moisture. Hence the most striking characteristic of this plant is the great tuber, which, like the bulb of the lily or the corm of the crocus, enables it to remain alive during the dry season. It is interesting to note that an important systematic character resides in this highly adapted structure, since in some species the tuber protects its contents by a corky layer, in others by a felt-like covering of hair. Its early development again is of great interest. The nourishment stored in a seed is generally used up on germination in the production of one or more green leaves, the assimilating organs, by aid of which the seedling becomes an independent organism. *Cyclamen*, however, has become so impressed with the importance of forming a tuber, that it starts even before the unfolding of the first leaf, which means that some of the reserve nourishment stored in the seed is not used to make leaf-tissue, but is passed down the leaf-stalk to form a new reservoir in the short stem just above the young root.

There has been much argument as to the cotyledons of *Cyclamen*. As a member of the primrose family, and, therefore, a dicotyledon, it

should have an opposite pair of seed-leaves. Gaertner, who first drew attention to this seedling, considered that the first leaf, to which we have referred, and which grows to a large foliage leaf, is the only cotyledon. Other botanists have taken the same view, while some maintain that a second cotyledon is developed later. Dr Hildebrand does not consider the nomenclature of these early-developed leaves a matter of importance, but carefully describes what happens. The development of the second leaf, presumably the second cotyledon, may be accelerated by removing the first. The author has had the inestimable advantage of observing the life-history of the species from seed to seed, and has been able to make many useful observations which would have been impossible if dried material only had been available. Among others we note an interesting correlation between the duration, or time of appearing, of the leaves and the length of the dormant period before the expiration of which this seed will not germinate.

The section on variation should be read by all who are interested in this subject, as bearing on the relation between variation and a changing environment.

The value of Dr Hildebrand's memoir is enhanced by half-a-dozen clear, double-paged plates, containing numerous figures.

The Female of Heterogyna

The recently published part 2 of the *Transactions of the Entomological Society* for the current year contains (pp. 141-150) another of Dr T. A. Chapman's valuable papers on the life-history of Lepidoptera. He describes the transformations of the South European *Heterogyna penella*—a small dusky moth with wingless female, often associated with the Psychidae which it resembles, but from its early stages apparently nearer to the Zygaenidae. The vermiform female imago remains attached to the ventral face of the pupa skin. She emerges from her cocoon for pairing, but withdraws into it again after fertilization, becoming replaced exactly in her former position in the pupa as before emergence. Oviposition then begins, and the pupa skin becomes largely filled with eggs, the shrivelled body of the female stopping up the aperture and protecting the eggs against drying up, as well as against insect parasites. Dr Chapman believes that the object of the return of the female into the pupa skin is to ensure this protection for the eggs. And maternal self-sacrifice is carried yet further, for the young caterpillars' first meal is on the remains of their parent. This concluded, they bore through the pupa skin and take to their food-plant.

I

Zoological Jamaica

THE last day of April 1896 found the Marine Laboratory of the Johns Hopkins University settled at Port Henderson, Jamaica. From the porch of the little cottage we occupied, one looks straight up Kingston harbour, where, seven miles away, lies the commercial capital of the island. Beyond the city rises Long Mountain, behind which the Port Royal hills show green and rugged; and, forming a background to the whole picture, tower the lofty Blue Mountains, with the Peak capping all. At early morning when the dawn comes up from behind that range, or late in the afternoon when the setting sun lights up with endless variety the ravines and gorges of the Port Royal hills, the scene passes description, and we constantly assured each other that a lovelier spot could not be found. Back of the cottage, the Salt Pond hills, surmounted by Rodney's lookout, separate us from the large salt-ponds, which lie a few miles down the coast.

The district around Port Henderson is one of the driest in Jamaica, and cacti form one of the most striking features of the landscape. These dry hills have a characteristic fauna, consisting largely of birds and lizards. The latter are very abundant, of half a dozen species, nearly all of which are handsomely coloured. At least two of them lay their eggs in the angles of the stem of the large cacti, called 'dildoes,' where they are safely protected by the thick, sharp thorns. Hermit-crabs and the large white land-crab are also abundant on these hills, and scorpions and centipedes are common under the rocks and logs. The white ants or termites are very common in the neighbourhood of Port Henderson and their large brown 'nests' are to be seen on all sides. These latter are excellent material, when dry, for a smudge to drive away mosquitoes, which are not wholly absent, though they seldom become much of a nuisance.

Although bats are occasionally seen at dusk and may be found in the caves during the day, the only common mammal, so far as our observations went, was the mongoose, which we often saw very near the house. The most interesting bird is a mocking-bird, peculiar to Jamaica, and said to occur at no other point on the island. The night-hawk, called by the natives 'Gie-me-a-bit' from

its very characteristic note, is common here, and occasionally the big black swift is seen near the summit of the hills. The ground-dove is very abundant and two or three other doves are common in the woods, so that their plaintive 'cooing' may be heard at almost any hour of the day. Swarms of cave-swallows breed in the numerous caves along the shore, their nests and eggs being very much like those of our common American barn-swallow.

The part of the harbour close to Port Henderson does not offer very good conditions for marine collecting, but some particular forms are abundant. The muddy and sandy bottom is literally carpeted with the commonest sea-urchin, *Toxopneustes*, and a black holothurian occurs in some numbers. The rocky shores are the homes of countless chitons of several species, and of a very lively crab, *Grapsus*, while a few species of gastropods are represented by numerous individuals. On the north shore of the harbour, which is sandy and slopes gradually into deep water, the huge star-fish, *Pentaceros*, which is so often seen as a curio in this country, is quite common, while large specimens of the shield-urchin, *Clypeaster*, called 'sea-moon' by the coloured boys, are occasionally met with.

Separated from the harbour proper by a narrow strip of sandy soil, there lies to the north quite an extent of shallow, somewhat stagnant, and near the mouth of the Rio Cobre at least, brackish water, in part filled up with mangroves. This is known as the 'Slashes.' The bottom here is stinking mud, and almost the only forms of animal life we found in the water were crab-larvae in countless quantities. Curiously enough it was in such a place that the late Dr F. S. Conant discovered great quantities of a small and exceedingly graceful Cubomedusa, which he has named *Tripedalia*. They were common among the mangrove-roots in water less than two feet deep, and they were not found anywhere outside of the 'Slashes.' The clumps and islands of mangroves are the homes of several species of herons and rails, the former called 'gaulins,' the latter 'mangrove-hens.' These swamps are rich collecting-grounds for the ornithologist, and, in the spring, for the oölogist as well.

Directly across the entrance of the harbour from Port Henderson, and a couple of miles away, lies the long low sand-spit, on the end of which stands the historic town of Port Royal. On the harbour side of this sand-spit are extensive clusters and islands of mangroves, among which are beautiful little bodies of water, connected by natural or artificial channels known as the 'Lakes.' The water in the Lakes is from two to twenty feet deep, and the bottom is usually more or less clear sand. On the roots of the mangroves, which hang down into the water on all sides, there is an abundant growth of sea-weed, with which are mingled oysters, ascidians,

sponges, and the like. No other spot that we visited in Jamaica could compare with the Lakes for abundance and variety of marine life. The sea-weed on the mangrove-roots is literally swarming with animals, and a single root would furnish profitable entertainment for hours. The sponges are of all colours and shades from black to white, red, green, blue and purple being the most striking. The oysters are small but abundant, and are said to be good eating. Beautifully coloured planarians and nudibranch molluscs are numerous both in species and individuals. One of the most interesting inhabitants of these colonies is a viviparous *Synapta*, which is abundant in certain parts of the Lakes. On dead mangrove-roots, where there is little or no sea-weed, one often finds large masses of an orange-red ascidian, which is one of the most noticeable objects to be seen. A large tubicolous annelid, *Sabella*, sometimes called locally a 'sea-hen,' is quite common, and is conspicuous on account of its large, brown and white tentacle-gills. Numerous crabs are found among the mangrove and in the sea-weed, and they form one of the most fascinating groups in the fauna of Jamaica. Over 100 species have been recorded from the island, and they differ so remarkably in size, shape, colour and conformity to their surroundings that there seems no limit to their diversity. Indeed, curious novelties in the crab line were sure to turn up on every collecting trip. We often found small fishes tangled up with the sea-weed when we lifted roots out of the water, and in this way we captured several specimens of the curious and graceful little sea-horse, *Hippocampus*. On the bottom of the Lakes the commonest animals are echinoderms, but one curious discomedusa, *Cassiopea*, is often found there, which, as it rests on the sand with its oral tentacles up, bears such a striking resemblance to a head of cauliflower that it is hard to believe it is not a vegetable growth. Several species of star-fish occur, the most striking of which are a bright red *Echinaster* and the large nine-armed *Luidia*. Two other species of *Luidia* occur, and one or two species of *Astropecten* are common. Ophiurids are represented by only a single species, and that a very small one, and sea-urchins are not very common. But holothurians abound, and are very noticeable. A few of these belong to the genus *Mülleria*, many more to *Holothuria*, but the great bulk of them to *Stichopus*. The latter show the most extraordinary variation in colour and form, so that the determination of species becomes a difficult matter. Large tectibranch molluscs, *Aplysiae* of several species, also occur in the Lakes, but they are not very common. They excrete a purple fluid into the water when they are irritated or disturbed. Sea-anemones are abundant and of several species; one genus, *Bunodiopsis*, is rather small, and covers the eel-grass in certain places with greyish-white patches. A large black ascidian is very common,

and forms quite a striking contrast to the white sand. Several species of fish occur, of which the mullet and the pickerel-like baracuda are the most sought after for food. Specimens of the curious, almost triangular trunk-fish are easily taken with a dip-net, in the shallow channels between the 'Lakes.' The remarkable fish, *Fierasfer*, was often found in the respiratory-trees of the large holothurians, especially *Mülleria*, and would only come out into the bucket or aquarium when the water became very impure.

Outside of the harbour, and several miles from Port Henderson, are a number of scattered islets known as the Cays. The largest of these are more or less overgrown with mangroves and other shrubs, but some of them are entirely bare of vegetation. They are all surrounded by reefs of coral, which are a source of unfailing interest to the zoologist. The variety in shape, colour and structure of the corals is delightful, from the massive brain-corals to the delicate feather-like alcyonarians. Inside the reefs are banks of sand, covered in most places by very shallow water, and overgrown in some spots with eel-grass. All over these sand-flats are slabs of broken coral-rock, on and under which are a great number of animals of different groups. This sort of ground makes very good collecting, Drunkenman Cay offering especial attractions. *Balanoglossus* of large size and of two or more species occurs there; and digging in the sand also brought to light several species of annelids, a small *Synapta* and a *Thallasema*-like echiurid. Nemerteans, planarians and sipunculids occur on or in almost every piece of coral, while sea-anemones abound not only among the crevices of the rock but in the sand also. Several specimens of a small octopus were seen, but they did not appear to be very common. The huge *Synapta lappa* lives under the slabs of coral, but it is not as common there as we found it to be afterwards at Port Antonio. Several other holothurians are common, including a very pretty brown and yellow *Stichopus*. Star-fishes of the genus *Asterina* are common, and show very remarkable differences in colour. They live closely attached to the underside of the rocks, and easily escape detection. Sea-urchins are represented by several species, the two largest being especially common. One of these, *Diadema*, is almost black, and has spines six or eight inches long, while the other, *Hipponoë*, is white with very short spines. The latter is sometimes eaten by the natives, and, as a matter of curiosity, we had our cook prepare some for us; but our curiosity was soon and effectively satisfied, and sea-urchins did not appear on our bill-of-fare again. Ophiurids occur under every piece of coral, and doubtless a great many species might be obtained by careful collecting. The most noticeable form is a large *Ophiocoma*, almost black, often marked with grey or white, and with numerous short blunt spines on the arms. Two large and very

curious species of lobster occur in the deep water outside the reefs, and though they differ much from each other, they are equally different from our American species. One is very slender and graceful with very long antennae, while the other is short, thickset and clumsy with very short antennae. Neither species has large chelae on the first pair of feet. The Cays are the resorts of numerous sea-birds, especially terms of several species which breed there. The graceful and handsome man-of-war bird is common around Kingston harbour, and roosts on the Cays, as does the brown pelican, which is quite common. One does not easily tire of watching pelicans fishing, so unerring is their aim, and so remarkable the force with which they strike the water.

Almost due west from Drunkenman Cay, the shore of Jamaica is a low beach of white sand upon which *Spirula* shells may be gathered at any time. Just back of this beach lie a series of three salt ponds, which must have been connected with, and probably were a part of the ocean until quite recently. The strip of land which at the present time separates the first and largest one from the ocean has increased in width very perceptibly in the last three years. The water seems to be much more densely saline in these ponds than in the sea, and animal life is far from abundant, presumably on that account. Gastropod shells occur in great numbers, but we found very few living specimens, and almost the only other animal seen was the large medusa, already mentioned, *Cassiopea*, of which a few small specimens were observed. Crocodiles and fish are said to be abundant, but we saw only one or two of the former and very few of the latter. One of the fish, known locally as 'Calipeerer,' is highly spoken of as a food-fish, being compared to salmon. We were told that this fish is found nowhere else in the island, that it is marketed in Spanishtown in the spring only, and that no other fish in Jamaica approaches it in quality. Unfortunately we could not verify these statements, as we were unable to get either sight or taste of this remarkable 'calipeerer.'

Inside Kingston harbour the surface-collecting offers a good deal that is of interest. Several large medusae (*Aurelia, Cyanea*), are quite common, while on calm mornings the very graceful cubomedusa *Charybdea* is not rare. We only noted one species of ctenophore, but that is a large and beautiful form, and is quite abundant. Two or three species of *Sagitta* are very common, and the curious decapod, *Lucifer*, fairly so. Crustacean larvae were abundant, but we found echinoderm larvae very rare, no matter when or where we towed. And this seems more remarkable when one considers that echinoderms are so abundant, and that many of the species were breeding; and, furthermore, echinoderm larvae were very abundant at the same place in 1891. Dredging gave

us very little satisfaction that summer, as the only specimens we obtained, which were not collected along shore at other times, were a small holothurian and a large spatangoid. The former was dredged off Port Henderson and proves to be an interesting and probably new species of *Holothuria*. The latter, *Meoma*, were dredged outside the reef at Drunkenman Cay, and were almost the only animals which the dredge brought up from the clear sandy bottom. In the lakes and in the slashes, very little animal life was brought to light, while in the harbour proper the dredge soon clogged with the enormous quantities of *Toxopneustes*.

From Port Henderson we made collecting trips to different places, two of which are worthy of special mention. The first of these was on the last day of May, and had Montego Bay and the Bogue Islands as its objective point. Aside from the opportunity to see the central and western parts of Jamaica, this excursion proved something of a disappointment. To the east of the harbour at Montego Bay, the shore is rocky and offers very much the same collecting as that at Port Henderson, while the only new animal which the sandy beaches afforded was a large white *Hippa*. On some of the reefs and on an old pile of masonry near the middle of the harbour, a tubicolous annelid occurs which is very noticeable on account of the shape and bright colours of its tentacle-gills. There seem to be two of these, one coiled on each side in a spiral, 15-20 mm. high. They were usually green, yellow, purple or red, but often these colours would be mingled with white, so that they were very handsome objects, resting apparently on the surface of the rock. If the rock was struck, however, they all disappeared as if by magic. The Bogue Islands lie a little distance to the west of Montego Bay, and as they are covered with mangroves, the sight of them arouses expectations of collecting like that in the Port Royal Lakes. But such hopes are soon shattered, for even a careful examination fails to show any superabundance of animal life. A large buff and brown *Stichopus*, the commonest species at Port Royal, is plentiful, and the same may be said of crabs, especially 'fiddlers.' Two species of star-fish were found, and a very few *Cassiopea*. Man-of-war birds were very abundant and much tamer than near Kingston. The roots of the mangroves were in many places well covered with sea-weed, but the expected animal life was wanting. Surface towing in the evening brought to light nothing of interest. We spent one night at Montpelier, about ten miles inland from Montego Bay, and provided with an excellent hotel. An early morning ramble along a mountain brook introduced us to some interesting crustaceans and arachnoids. Small black crabs, the females of which were carrying eggs, were common among the stones, but like the shrimps, which were abundant in the

pools, they were hard to catch. Spiders of small size were common, and one large *Phrynus* was captured, carrying about a dozen large eggs on the under side of the abdomen.

Our second excursion made late in July was up Blue Mountain peak, and it was a pleasure and success far beyond our expectations. The changes in the flora as one goes upward must impress even an unbotanical zoologist, and the magnificent tree-ferns cannot be passed by in silence. As one gets well up into the mountains the clear whistle of the solitaire, not unlike that of our wood-thrush, is sure to attract attention. Some handsomely coloured finches were seen in the deepest woods, but birds did not seem to be abundant. On the summit of the peak we made a careful search for the eggs of a small tree-frog that is common there, and we were rewarded by finding many. They are laid in clusters of a dozen or more in the wet moss which covers everything. Each egg is two or three millimetres in diameter, and seems very large for the size of the animal. The scientific worker, no matter what his specialty, who visits Jamaica and fails to make the journey up Blue Mountain peak, misses one of the most charming features of the island.

In 1897 the University established its laboratory at Port Antonio, the most beautiful harbour on the north side of the island. From here trips were made east and west along the coast, and inland to Cuna-cuna Pass, Castleton Gardens and Bog-Walk. Members of the party also visited Porus and Mandeville in the centre of the island, and one party made a week's trip around the west end of the island, visiting the harbours of St Ann's Bay, Rio Bueno, Falmouth, Montego Bay, Lucea, Saranna-la-mar and Black River. The marine fauna along the whole of the north shore seems to be essentially the same as on the south side of the island, but the land fauna differs very much from that near Port Henderson. There are no mangrove swamps on the north side in any way comparable to those at Port Royal, but the collecting on the reefs is very good, and strikingly like that on Drunkenman Cay. The same star-fish, sea-urchins, ophiurids, and holothurians occur, but are more abundant and easier of access at Port Antonio. The same may be said of most of the annelids, molluscs and sea-anemones, and is especially true of the corals. Deep water is so very near to the shore that the 100 fathom line is within easy rowing distance. The richest collecting is on the sandy bars or flats in one to three feet of water, where are plenty of slabs of broken coral rock, under which an abundance of animals is sure to be found. One of the most interesting of these is a small flesh-coloured holothurian, *Chirodota*, several specimens of which were collected with the body-cavity full of young. While collecting on these flats, the small but numerous fishes, many of them gorgeously coloured, proved a great annoyance,

as the moment the rock was overturned they would rush in and seize any hapless worm or other small, soft animal which might be exposed; many fine specimens were lost in this way. Several holothurians were found at Port Antonio which were not seen elsewhere; one of them was remarkable, not only for its large size and unusual facility of locomotion, but also for its habit of eviscerating as soon as brought to the surface of the water, so that it was impossible to procure perfect specimens. Several species of echinoids, not seen on the south side, were found here, but *Toxopneustes* was comparatively rare, only a few small specimens being found. The large brown *Echinanthus* was not uncommon, and the huge *Mcoma*, which we found common near Drunkenman Cay, was plentiful and was twice the size of those we saw on the south side. Two other very pretty little spatangoids, *Brissus* and *Echinoneus*, were very common in the same situations with *Chirodota*. The commonest sea-urchin was the dark reddish-brown *Echinometra*, which simply covered the rocks in some places. Some very handsome gastropods were collected on the reefs, and the large conch, the shell of which is so common in America for ornamental purposes, occurs plentifully all through the harbour. Along the sandy shore, near the lighthouse, the same white *Hippa* which we saw at Montego Bay is common, but on the shore of East Harbour, where the sand is mixed with black mud, occurs a somewhat smaller dark-brown *Hippa*, which differs from the other in habits as well as in appearance. Dredging proved more interesting than at Port Henderson, especially in East Harbour where there is a good deal of eel-grass on the bottom. Here we found large numbers of the beautiful Cubomedusa, *Charybdea*, which we rarely saw at the surface, and in the same locality the delicate olive-green sand-dollar, *Mellita*, is quite common.

The fresh-water and land fauna at Port Antonio is especially interesting on account of the numerous streams and the proximity of the hills. The streams abound with gastropods and shrimps, and large, beautifully coloured cray-fish are common. Insects are not particularly numerous or noticeable, but myriapods and arachnoids are abundant. Pseudoscorpions and pedipalps are both plentiful, and scorpions are not rare. Large centipedes with their yellow eggs, two or three millimetres in diameter, were frequently brought to us by the coloured boys, while equally large millipedes were common in the woods. Only a single specimen of *Peripatus* was found, and that was a small one; but we were told that they were not considered exceptionally rare. The land-crab at Port Antonio is very different from the one at Port Henderson, the two being distinguished as the 'black' and 'white' land-crab, respectively. Land-molluscs, especially large slugs, are very common, and the eggs of

the latter were often found in the banana 'trash.' Tree-frogs are common, especially the species which lays its eggs in the water held in the base of the leaves of the *Bromeliae*, where we often found the tadpoles swimming about. Lizards of all sorts and sizes abound, some of them being beautifully coloured. Two or three small iguanas were also brought into the laboratory, but they are rather rare. At Port Antonio we heard a good deal about Jamaica's most interesting mammal, the agouti, or, as it is more often called, the cony. It is peculiar to the island, though there is an allied species in Cuba. Though now quite rare it is still to be met with in the John Crow and Blue mountains. There are several specimens in the small menagerie of native animals kept at the Jamaica Institute in Kingston, and they have bred there. At Port Antonio we were offered a pair alive for twenty dollars, but we were unable to procure any, dead or alive, at any less price.

Most noticeable and best known of all the native fauna, the birds of Jamaica demand a special word. Over 200 species have been recorded, of which 40, almost exactly one fifth, are peculiar to the island. Of the remainder about 50 may be classed as West Indian, while about 90 are distinctly North American, many of our common New England birds being migrants or winter visitors. There are four or five summer visitors from the mainland of South and Central America, but they form a very insignificant part of the avifauna. Much the greater number of water-birds are more or less well known in the United States, and the same may be said of the warblers. But the doves, cuckoos, swifts, humming-birds, parrots, and fly-catchers are almost exclusively West Indian, and a large number of them are distinctively Jamaican. On the coast, besides the man-of-war birds and pelicans already mentioned, the tropic-bird occurs and terns are abundant, especially in Kingston harbour. The two common humming-birds are found in all cultivated districts; one is interesting because of its very small size, the other because of its rich, dark-purple plumage and long, forked tail. About Kingston the small palm-swift is abundant, and nests in the cocoa-nut trees in the very heart of the city. The most gorgeously coloured bird, the tody, is one of the smallest, and its bright red and vivid green plumage is very striking. The two fly-catchers, which correspond so closely to our king-bird and phoebe as easily to deceive even a careful observer, are especially common at Port Antonio, where the peculiar cuckoo, *Crotophaga*, is also always in evidence.

It is curious to note the paucity of names among the natives for even the common animals, a single name being made to serve for two or more widely different forms. The name 'sea-cat' is given to the octopus, and to large medusae, especially *Cassiopea*,

but it is also used almost indiscriminately for any unusual fish. 'Sea-squirt' is used for both ascidians and holothurians. When a native speaks of an 'owl' or a 'potoo,' it is not always possible to determine whether he means the barn owl, or the large goatsucker, *Nyctibius*, as the names are constantly interchanged. 'Rain-bird' may be a swift or one of three different kinds of cuckoo, while 'doctor-bird' and 'banana-bird' are also terms whose value depends on the speaker.

That the fauna of Jamaica is undergoing comparatively rapid changes must be clear to even a casual observer. The manatee and the agouti, as well as the iguana, are apparently on the road to extinction, and it is almost certain that the famous 'Blue Mountain duck' (*Aestrelata caribbaea*) has been exterminated during the last half of this century. The introduction of toads and the mongoose have clearly affected the land fauna, though it looks as if the native animals were adapting themselves to the new conditions. It is only a little over twenty-five years since the mongoose was introduced, and it is now common everywhere. Five or six years ago the snakes seemed to have been practically exterminated by their new enemy, but now they are beginning to appear again, so that one or two species are no longer varieties near Kingston, and we saw several near Port Antonio. Whether the snakes have developed some new form of defence or escape, or whether the mongoose has ceased to look for food in that group of reptiles, is still an open question. It is not only among the land animals, however, that such changes are going on. Several instances of remarkable changes in the abundance of a given species may be mentioned among marine animals. There seems to be very good evidence showing that the viviparous *Synapta* is becoming rarer each year, and that the area it inhabits is becoming more and more restricted. *Cassiopea* was far from common in 1896 where it was most abundant in 1893, and we did not find large numbers of it in any one place. The remarkable little cubomedusa, *Tripedalia*, which was very abundant in the 'Slashes' in 1896 had completely disappeared in 1897, and a week of careful searching failed to disclose a single specimen anywhere in the Slashes or Lakes. It may not be safe to draw any sweeping conclusions from a few isolated facts of this sort, but they are at least worthy of note.

One cannot close an account of zoological Jamaica without some reference to the scientific work which is being done in the island itself. As an authority on Jamaican shells, Henry Vendries, Esq., of Kingston, has a world-wide reputation, and his collection of native shells is very extensive. Mr P. W. Jarvis has been an extensive collector of the crabs of Jamaica, and has furnished the United States National Museum with the types of many new

species. Mr C. B. Taylor has collected and studied birds in all parts of the island, and is beyond question the best informed man on the island in questions of ornithology. For the past nine years natural science in Jamaica has enjoyed the patronage and support in countless ways of His Excellency Sir Henry Blake, the Governor of the island, and his estimable wife. Lady Blake has painted from life the caterpillars, chrysalids and adults of many of the native Lepidoptera, and her collection of over 100 water-colours of this order are a treat to the artist as well as to the entomologist. She has also painted many of the beautifully coloured fish which abound in the Caribbean Sea, and she has contributed to scientific journals in America and England, articles on the "Aborigines of the West Indies" and kindred topics. But the scientific work in Jamaica naturally centres around the Jamaica Institute at Kingston. A handsome building, erected only a few years ago, houses a very good collection of the principal animals of the island, some of the specimens being of considerable value. At the rear of this building is a small zoological garden, which contains specimens of many of the most interesting native birds, mammals, and reptiles. There is at the Institute a very good library which is of great assistance to a working zoologist. A specialty is made of books on Jamaica. The Board of Governors of the Institute show every courtesy to visiting zoologists, and are ably seconded by the present curator of the Museum, Mr J. E. Duerden, a Dublin University man, himself a trained zoologist, whose work on the Actinaria of the island is already attracting attention. An excellent beginning has been made towards interesting the people in the natural history of their island and in making the museum the repository of a complete collection of the native fauna. In both of these aims the Institute deserves and ought to receive the assistance of every scientific visitor to the island.

HUBERT LYMAN CLARK.

AMHERST COLLEGE, MASS., U.S.A.

II

The Eskers of Ireland

OF the more recent geological phenomena none are more curious, and none have given rise to more speculation, not to say controversy, than the ridge-like accumulations, principally of sand or gravel, found throughout the midland district of Ireland.

Considering that geologists have very commonly associated these ridges—eskers as they are called in Ireland—with the products of glaciation, it appears to me not a little remarkable that they are confined to a comparatively narrow zone running through the flattest part of the island from Galway Bay to Dublin Bay. The remark would apply with almost equal force to the corresponding formations of Scotland and the Scandinavian peninsula. The kames are nearly confined to the valleys of the Clyde and Forth, as the åsar of Sweden have their most striking development in the Lake Mälar district. True, there are mounds of gravel in some of the northern counties of Ireland, but they are not to be confounded with the typical eskers of Galway, King's County, North Tipperary, Queen's County, Kildare, and Dublin; and in Scotland the term kame is applied to ridges and mounds "of marine, lacustrine, fluviatile, and meteoric (wind-driven) drifts."

"The centre of Ireland is chiefly a great plain of Carboniferous Limestone, partly surrounded by several groups of lofty hills composed of the oldest rocks, which rise from beneath the limestone. The hills to the south of this plain have every height up to 3000 feet above the sea. Other hills, rising to heights of 800 or 1000 feet, are composed of Coal-measures lying on the limestone; these are surrounded by valleys which are branches of the great plain. The general level of the limestone plain is from 100 to 300 feet above the sea, only a few isolated hills of limestone in the interior of the country rising to as much as 500 or 600 feet."

From this description by Jukes, it is clear that, if the surface of the country were depressed 300 feet from its present level, the waters of Galway Bay would meet those of Dublin Bay, forming a broad channel interrupted only by a few islands and occasional shallows. That such was the case during at least a portion of the period of the deposition of the limestone-gravel is generally maintained by Irish geologists; and the term 'esker sea' (Kinahan) has been used to denote the inland waters when the land depression was at or near

300 feet from the existing level. The old sea-margins are pointed to in proof of the depression—lines of beach, and notches cut into the hill-sides; but, as will appear further on, such evidence is by no means conclusive of marine action.

"The low country," continues Jukes, "is largely covered by a widely-spread mass of drift, consisting of dark sandy boulder-clay, with pebbles and blocks, and occasional beds of sand and gravel, sometimes very regularly stratified." In a footnote he adds:—"This seems to be the equivalent of the Scottish *Till*, at least in its upper part." The gravel, it may be remarked, extends throughout wide areas and to considerable depth, without any apparent mingling of clay, but consisting wholly, or almost so, of sand and pebbles. "The great majority of the pebbles are rounded fragments of Carboniferous Limestone, whence the deposit usually goes by the name of the limestone gravel. This deposit rests not only on the limestone but sweeps up on the flanks of the hills, both those that are made of the lower palaeozoic rocks and those formed of the Coal-measures. In each case the limestone gravel becomes largely mingled with detritus of the rocks of which the hills are made, and sometimes to such an extent that the local rocks assume a decided preponderance and occasionally compose almost the whole of the deposit."

That these superficial deposits were formed under the sea, Jukes entertains no doubt, just as Professor Ramsay holds that the drift of North Wales is of marine origin. Kinahan agrees with Jukes. The only ground, according to these able writers, for a contrary opinion is supplied by the unfossiliferous character of the deposits. But the circumstances would not be in favour of the preservation of shells (save those embedded in the limestone) in a mass of materials, which presumably have been subjected to much agitation and trituration. Against these, however, we have the very decided opinion of Dr James Geikie, as to the analogous order of things in Scotland:—"It seems most reasonable to conclude that neither the water-worn and stratified drift, nor the loose angular debris, nor yet the erratics that lie scattered over the low grounds of Scotland, give any indications of a former submergence of the land below the sea. The loose angular debris or moraine matter and erratics have been carried down and dropt over the terminal front of the ice-sheet, or have stranded upon the mountain-slope and hillside, or have been left lying in what once formed the bed of the old ice-sheet. The sand and gravel drifts have been produced by the action of water escaping from the melting ice-sheet which re-arranged the morainic debris, &c., heaping it up in banks or spreading it out in undulating flats" ("The Great Ice Age," p. 244 of 2nd Ed.).

Passing, for a little while, from this direct conflict of opinion, we come to discuss the particular subject now before us. For the present we accompany Jukes: "One of the most remarkable features of the upper gravelly and sandy drifts is the way in which these deposits are often heaped into mounds and ridges, which sometimes run continuously over the surface of the country. Such ridges are known as kames in Scotland, eskers in Ireland, and åsar in Sweden. These remarkable outlines are not due to mere denudation, but, as shown by the external structure of the mounds, have usually been produced at the same time as the mass of the sand and gravel was deposited.

"These mounds, in most cases, probably received their form during their first accumulation; but sometimes the surface of the drift seems to be one caused by subsequent erosion." "In one conspicuous instance (Jukes adds), two or three miles north of Parsonstown, which I visited in November 1861, in company with Mr A. B. Wynne, a widely spread expanse of deep horizontally stratified limestone gravel appears to have been so far acted upon by subsequent denudation as to have now an abruptly-undulating surface consisting of small mounds, ridges, and valleys running in various directions over a space several miles in length and one or two in breadth. One of these ridges, however, and the most conspicuous of them, forms a long esker, or narrow gently-undulating bank some fifty feet above the surrounding flat country, and some miles in length. Such eskers are very numerous in Ireland over all the low central plain. One is to be seen three or four miles to the west of Dublin, running from the banks of the Dodder, past the old castle of Tymon, by the Green Hills, towards the valley of the Liffey." Jukes mentions similar accumulations at Maryborough, Stradbally, Borrisokane, and other places.

One of the most remarkable groups (although not mentioned in particular by either Jukes or Kinahan) is situated at the western terminus of the series, just three miles to the south-east of Athenry, at St Joseph's College—locally known as 'Esker College'—formerly a Dominican priory, and so marked on the Ordnance Survey map, but lately converted into a diocesan college. The locality is called 'Esker,' just as—to the great perplexity of post-office officials—several other townlands to the west of the Shannon are so named owing to the prevalence of the drift ridges. But 'Esker, Athenry,' is distinguished among them all, no less by its history than by the gigantic character of its ridges and mounds of limestone gravel, a subject which I shall have occasion to discuss in some detail further on.

I quote again from Jukes: "The eskers are often opened for gravel pits, as may be seen in the Green Hills, near Dublin, and

the arrangement of the materials is very curious. Irregular beds of large blocks, or of small pebbles, or of the finest sand, are arranged one over the other, generally with a rude attempt at conforming to the external slopes of the ridges, but not preserving to any distance either the thickness or the inclination."

This description of the 'section,' although written with special reference to a ridge quite at the eastern extremity of the great central plain, would apply to the great esker at the College near Athenry, particularly the remarks on the stratified arrangement of the sand and gravel.

The true esker, or ridge, when seen at a little distance bears a striking resemblance to a railway embankment, and, as Kinahan remarks, is sometimes so narrow at the top that people may almost shake hands across the width. This is, however, rather exceptional. I know well the Parsonstown esker described by Jukes. It crosses the county road between Birr and Banagher—the road is, in fact, cut through 'The Ridge'; and the latter runs across the country in the direction of the Shannon, the top of it serving, for a considerable distance, as a bog-road or boreen. The Maryborough esker —also locally known as The Ridge—is said by the country people to extend "all across Ireland": it can indeed be traced, more or less continuously, for many miles. Adjoining the town of Maryborough its slopes and top have until recently been used as a cemetery. Geikie mentions certain kames in Scotland that had long been used for the same purpose.

So far we have been making approach to the interesting but perplexing question—How came these eskers to be what they are? By what particular agency, or agencies, have sand, gravel, clay, and shingle been ridged up, and at the same time sorted and stratified as we find in the typical esker?

All who have attempted the solution begin by confessing the very great difficulty of the question; and the admitted difficulty has given rise to a considerable amount of 'scientific' romancing.

"A small mound quite close to Dunfermline is locally famous under the name Mont Dieu. According to an old story this drift mound owes its origin to some unfortunate monks, who, by way of penance, once carried the sand in baskets from the sea-shore at Inverkeithing" (J. Geikie, "Great Ice Age," p. 212). And there is a similar legend as to the origin of a mound in the valley of the Kali Water, Roxburghshire. From the economical point of view these cases are very good examples of 'unproductive' labour. But, from the inquirer's point of view, they are hardly more romantic than the explanation put forth, in the cause of science, by Mr A. E. Törnebohm as to the origin of the Swedish åsar or eskers.

His belief is that the åsar are ancient river courses, and he

points to their river-like ramifications. In the valleys containing these åsar, detached patches of sand are sometimes found—the wreck, he believes, of a great deposit of sand. In such a deposit, rivers would soon cut down a deep channel. In the bottom of this channel pebbles and gravel would collect, then gravel, or sand, or mud layers. In time, the course of the river is diverted, the adjacent sands wear rapidly away, and the more compacted deposits of the river-bed longer resist denuding influences, and in the end stand out in bold relief as åsar or eskers.

One can hardly forbear exclaiming—Too clever by half! The author uses, to the full, the romancist's privileges. He has everything he requires at hand and in abundance, and what he no longer requires he gets rid of with the readiness of a necromancer. Rivers have, no doubt, often struck out new lines of action for themselves. But here everything makes its bow and retires just when it has acted its part.

THOMAS FITZPATRICK.

ST IGNATIUS' COLLEGE,
GALWAY.

(*To be continued.*)

III

The Chemistry of the Forest Leaf

THE fundamental feature of a leaf is, as everybody knows, the presence therein of a body called chlorophyll, which is defined as "the substance, or maybe a mixture of substances, to which the pure green colour of ordinary healthy leaves and of other vegetable organs is due." Here our attention is at once arrested. Is it possible that no competent and expert chemist can be found who will proclaim with absolute confidence and assurance that chlorophyll is a single substance, or that it is a mixture of substances? So far as I can find, the original investigator of chlorophyll was Morot, and he gave a hypothetical formula for it, but said it was always accompanied by a fatty substance which he regarded as the chromogen thereof. In 1860 Fremy showed that the green matter was a simple mixture of two bodies, viz. a blue (phyllocyanin) and a yellow (phylloxanthin) existing side by side; whereas Pringsheim and others held that these were merely decomposition products of an originally single chemical individual. On the other hand, Dr Sorby considered the existence of a chlorophyll, a phyllocyanin, or a phylloxanthin of a definite chemical composition to be improbable. Meanwhile, an examination of the foliar organs of various species among the great vegetable groups and classes led Gautier in 1866 and later to conclude that chlorophyll differs among these—nay, even in various species of the same genus it may be dissimilar. But the crowning consummation of all previous researches seems to have been reached in 1895 when Mr Etard, in a memoir read before the French Academy, declared that a given species of plant may contain several chlorophylls, *e.g.* he describes four distinct, perfectly defined ones as occurring in Lucerne. "It may be concluded," he states, "that the green matters of leaves contain a very stable fundamental nucleus carrying the function of optical absorption in connection with a biological process; and around this nucleus, this trophic point, can be fixed, in a way more or less permanent according to the needs of nutrition, different chemical groupings giving place to chlorophylls different in their composition, their molecular weight, their solubilities, their isomers, and the rôle which they may play in the living species." "The operations of vegetable synthesis leading at the same time to fatty bodies insoluble in water and to matters eminently soluble, always by the intermedium of absorptive green matters, it is natural to think that one and the same chlorophyll would not be sufficient for the work" (*Comptes Rendus*, vol. cxx., p. 328).

Now, although Mr Etard has, by virtue of masterly analytical skill, succeeded in separating certain distinct green bodies occurring in the same species, which seem to respond to a definite chemical composition, it is not so clear that the question as to what chlorophyll itself really is has been finally settled. The grand difficulty is to decide definitely whether chlorophyll is (like carotin) a solid substance in itself and present in such quantity as to allow of its being separated and withdrawn purely and simply from the other constituents of the leaf; or whether it exists therein in exceedingly minute quantity only (like that of a dye fixed on a vegetable fibre), and inseparable therefrom save by means which bleach and destroy it irrevocably? It is evident that if the former alternative be accepted, then by simply absorbing the green pigment by animal charcoal from the 'chlorophyll,' it will still retain its physical and chemical characters unchanged; if the latter be adopted, the same absorption will completely destroy only the colouring matter as such.

Unfortunately it is absolutely impossible to tackle the question on these grounds or in this fashion, for the simple reason that no solvent at present known extracts chlorophyll, *i.e.* the pigment, purely and solely from the leaf; it is invariably conjoined with fats, waxes, resins, etc., and even subsequent exhaustive treatment with a series of different solvents does not eventually effect a complete purification. Such being the case, we must rest content with the results and effects of close application and a prolonged experience, feeling assured that in the course of time views and vistas will open, leading to a correct apprehension of the subject. One of these views may now be exploited for the edification of the reader.

Microscopic observation of the living leaf reveals that the chlorophyll granules are individually independent globules of dense protoplasm without proper walls plunged in the midst of the fundamental protoplasm and tinged by the green matter, their form and size remaining unaltered when extracted by ether, etc. Protoplasm itself is of course insoluble in the solvents which readily dissolve chlorophyll, but it is always accompanied by fatty matter of free formation which doubtless is one of the products of assimilation. Starch also, although not invariably present, very commonly occurs in these corpuscles; and Belzung seems to think that its presence is necessary for the formation of the green pigment—an opinion which I am disposed to dispute. In addition, however, to starch and fat, the occurrence of which is sufficiently palpable, it would be absurd to imagine that other bodies are not evolved in quantities more or less minute during the career of the tremendous vital energy exerted by the living protoplasm. Tremendous certainly it must be at the actual moment of its outcome, but perhaps for that very reason essentially frail and subject to degradation and decay. Hence arise

the well-known and characteristic decomposition products, normal or putrefactive, one of which, to take an example, indoxyl-sulphate of potassium (present in human urine) is easily oxidised to the powerful pigment indigo. In 1873 Bommer showed that some very small colourless granules scattered on the protoplasm of the flowers of *Phajus maculatus* readily produce indigo when the oxygen of the air comes in contact with the cell-contents; and according to him the experiments of Fremy fully suffice to prove that the origin of blue chlorophyll (phyllocyanin) is due to the presence of indican transformed into indigo, which exists in chlorophyll though in excessively minute proportion. We may observe that it is just this extreme minuteness which forbids probably for ever the precise determination of the chlorophyllian chromogen. We do not know what its chemical constitution is, or whether it is a real dye-stuff with a function either basic or acidic (I believe it is the latter). It has been maintained that chlorophyll is not a substance derived from an open chain of carbon atoms, and that very probably it is a derivative of a benzenoid hydrocarbon; but I think it is more probably a derivative of polyuric or acrylic acid. The difficulties attending the discovery of its real origin and chemical relationships seem to spring from the fact that its outcome and presence in the leaf do not depend on the aërial oxidisation incident to an expanded cellular surface, still less on transpiration, but are solely and absolutely the direct and immediate result of the life energy whose existence is indissolubly interwoven with the protoplasmic stroma of the chlorophyll granule.

A. Baeyer suggested that in the dissociation of carbonic acid, or of water, protoplasm is at the bottom of the whole business, and that chlorophyll plays only some subsidiary and indirect part, such as that of temporarily fixing carbon dioxide as does haemoglobin, and so facilitating the dissociation. According to Engelmann, while the chlorophyll granules give off oxygen in the light, colourless protoplasm, cell membrane, and nucleus do not; the green pigment itself is not capable of so acting, it must be present in connection with the living stroma of the chlorophyll granule. Timiriazeff held that the function of chlorophyll is to absorb the rays which possess the greatest energy, being transparent for those rays, and to transmit this energy to the molecules of the carbon dioxide, which would not of themselves undergo decomposition; while Mr Berthelot has shown that the presence of inert gases, such as nitrogen in the air, determines the phenomenon of the dissociation, they having the effect of separating the atoms of the carbon dioxide, acting thus on the active gas as a diminution of pressure would do. Nevertheless, and although all these physical and mechanical agencies may aid and abet, the question is, are they really and truly the practically determinant or dominant causes of the effect? It seems to be tolerably certain

that colourless protoplasm is quite competent to effect the dissociation in question (*i.e.* if such ever actually does come to pass), provided always that its forces are sufficiently concentrated and its position favourable. It is known that the protoplasm of the chlorophyll bodies is, like that of the nucleus, denser than the cytoplasm, and this increase of density is doubtless associated with an augmentation of vital energy and activity. Then why may not the nucleus with all its superior density likewise achieve the decomposition of carbon dioxide? Because its composition is utterly different from that of the cytoplasm; its function is to generate not special matters, but special motions; it excites the activity of the cytoplasm, but is not indispensable for metabolism; it is no part of its business to decompose gases or liquids; in fact, according to Preyer and Windt, it merely regulates the progress of the assimilation and de-assimilation of the protoplasm. In this connection it may suffice to merely mention the amyloplasts of Schimper, the leucoplasts of Sachs, and the free formation of starch without the intervention of chlorophyll, or even of leucites demonstrated by Belzung.

If we profoundly consider the matters herein and just now set forth, we can hardly resist the conclusion that chlorophyll, *i.e.* the simple pigmentary substance, is not truly the cause or indispensable auxiliary of the assimilative energy of the protoplasm of the leaf, but rather it is one of the direct and immediate consequences of the vital activity thereof. Whatever increases and heightens the vital activity increases at the same time and *pari passu* the amount of chlorophyll. The peculiar and characteristic feature in connection therewith is that it is not really a waste-product, or an excretion in any sense, or a product of de-assimilation; it is a specific consequence of the specific life activity interwoven with a protoplasmic substratum, whose molecules dissociate but do not degrade. Living protoplasm has an active power of respiration, and the greening of etiolated leaves grown in the dark is, when they are exposed to light, for and by itself no assimilation process, and it takes place without decomposition of carbon dioxide. The pigment itself is almost certainly a dark dull blue substance, such as can be isolated from Gramineae more especially, and the mixture thereof with the brilliant and stable yellow carotin, which all leaves contain, affords the greenery, and at the same time vivifies and, as it were, burnishes it.

If Mr Etard's conclusions that a given species of plant may contain several kinds of chlorophyll be provisionally accepted, it is indispensable that the term 'chlorophyll' should mean not the pure pigment only, but the entire waste or excretion ensuing from the chemical processes undergone in the protoplasm of the chloroplastids. His researches furnish a twofold basis. "It is the chlorophylls," he states, "that are insoluble in pentane, which tend on splitting up to

produce carbohydrates, tannins, and extracts ; those that are soluble in pentane tend to produce essences and oils." Hence supposing that these two chlorophylls exist in different or varying proportions in different species of plants, and practical analysis proves that they do so, then we have a satisfactory explanation of the fact that some plants are oil-producing, while others are starch-producing. It is well known that the leaves of various species differ enormously in their capacity for forming starch, and trees have actually been classified by Fischer and Suroz into 'Fettbäume' and 'Stärkbäume.'

There is, however, another view of the matter, viz., that propounded by Mr Mesnard, according to which, while the free fat oil, carbohydrates, and the reserve albuminoids are products of assimilation, the tannoid compounds, essences, resins, and tannin are products of de-assimilation. All are products of chlorophyll, and all constitute materials for the latex ; but the immediate origin of the fixed oil is, if this version be correct, not the same as the immediate origin of the essential oil ; whereas, according to Mr Etard, both of these plant constituents proceed from the same kind of 'chlorophyll.' It is hardly necessary to mention that the original doctrine formulated by Wiesner, and upheld by Fluckiger and Tschirsh and by many other German chemists and physiologists, was that the essential oils and resins are formed at the expense of starch or even of cellulose, that in fact the resins and ethereal oils are the final products of a series which begins with starch or glucose, and passes through tannin as an intermediary (Franchimont had, however, contested this as to resin) ; but according to Mr Mesnard the substances in question proceed from the transformation of the tannoid compounds produced by chlorophyll. "The formation of the essential oil," he states, "is very rapid, and is effected in a fashion almost immediate, while that of the tannins and pigments requires a long exposure to light and air."

Thus it will be seen that while many investigators, with some honourable exceptions, were fumbling and bungling over the subject, and allowing themselves to be misled and distracted by fanciful formulas and mystical alchemy, the true scientific ideas and experiments of a single Frenchman (with whom, however, Mr Blondel may be associated) have thrown a powerful search-ray of light over the hitherto dark and troubled waters. Personally I never could swallow the doctrine that the tannins were derivatives of the carbohydrates ; and notwithstanding Waage's elaborate formulary to prove that phloroglucin is formed as the result of the splitting up of glucose, I am more disposed to accept Nickel's view that it may be formed by the withdrawal of water from inosite—a body whose reactions seem to connect it to some extent with the proteids or albuminoids. I am convinced that, save and except the processes connected directly with assimilation, the other subsidiary and supplementary chemical

operations which take place in the leaf are of a comparatively simple character. Before, however, we proceed further, it will be advisable and indeed highly requisite to provide a table after Mesnard (slightly altered):—

Products of assimilation.	Products of de-assimilation.
Free fatty oil.	Tannoid compounds (rutin, etc.).
Carbohydrates (starch, glucose, etc.).	Resins, balsams, and essential oils.
Albuminoid matters of reserve.	Tannins.
	Coloured pigments.

It would be superfluous to adduce facts as bases for arguments to prove that the sketch here set forth is a positive and veritable representation of what actually comes to pass in the chemistry of the living foliar organ. No one disputes that fats, carbohydrates, and albuminoids are direct products of assimilation. The fat oil is deposited in the cells without there being a simultaneous deposit of albuminoid matters; but that there is some close connection between the oil and the starch and sugar is evident (though only provisionally and superficially) from the respective distribution of these bodies in the plant tissues. Thus the oil of the ripe seed is, according to Sachs, produced from the starch and sugar transported from the primary stem. The storing up of oil occurs in all the amylaceous tissues. In the autumn the starch contained in the branches of our forest trees is gradually 'transformed' into oil, and in the spring this oil is again 'transformed' into starch and sugar. The cells of the leaf which are free from oil are also free from starch. On the other hand, the fact that oil occurs stored up in the cells of the perisperms or of the cotyledons of certain seeds in Cruciferae, poppies, flax, almonds, etc., in which little or no starch is produced, would seem to show that oil and starch are more or less independent of each other. "In all cases," says Mr Mesnard, "the fatty oil is independent of the starch and the glucose, even in the rare cases of grass seeds where the oil is specially localised." I think it by no means follows because, as in the winter boughs of our forest trees, the oil steps into the place of the vanishing starch, and *vice versa* in the spring, that therefore one of these bodies is unquestionably derived from, transmuted into, or even formed at the expense of the other. Protoplasmic activity specifically directed is amply capable of creating or of destroying one or the other constituent independently and in accordance with the contemporary needs of the organism. Possibly may it not be that the winter production of oil is associated with low vitality, while the summer production of starch is associated with a high vitality? With regard to the organic acids of the leaf, I adhere to the views of Dehérain and others that they are oxidation products of the carbo-hydrates, and have got nothing to do with the synthesis of the proteids.

With reference to the scheme of the products of de-assimilation exhibited on the right-hand side of the aforedrawn table, it may be observed that a considerable experience in plant analysis is requisite in order to appreciate it thoroughly. Suppose, for instance, that we are determined to engage in a complete study of the chemistry of the forest leaf, we must commence with the earliest growth of the organ—in fact, we should commence with the winter buds, but at any rate we examine the young leaves just unfolded, and we, by appropriate methods of analysis, discover that one body seems almost universally present in them all, and that body is a tannoid compound (rutin or quercitrin). It is inevitably present because it is the first formed product of the chlorophyll (*i.e.* the proteid) substance that has spent its energy, and lies in the position of a waste or excretum. But what then ensues?

The leaf progresses in growth. Oxidation more or less complete supervenes on every item of its tissues and contents which is not directly under the dominion of the reducing energy of the living and vigorous protoplasm. The tannoid compound aforesaid is gradually transformed more or less completely into volatile oil and resin, or into tannin, but until the late autumn the change into the latter body is probably never entirely and absolutely accomplished. Practically in the majority of our forest trees there is no formation of essential oil in the leaves, but there is always apparently a residuary resinous body or a 'bitter principle' extracted by benzene or alcohol, and there is invariably tannin, the quantity of which increases from spring till autumn, reaching its maximum about October or November. According to Kraus, there is generally twice as much in the October leaves as there is in the June leaves. That some such progressive change ensues as is here described, even supposing that a portion of the material formed is daily conducted away into the bark and wood, there can be no doubt whatever.

On the whole, therefore, it may be admitted as established that, while the forest leaf is *par excellence* an organ of reduction so far as its purely and distinctively protoplasmic energy is concerned, yet at the same time very considerable and important oxidising processes take place among the lifeless ruins, so to speak, of its spent and exhausted activities. The bye-products, the waste of protein bodies are only partially and slowly excreted and cast off, an expulsion which dies gradually away in the old age and decrepitude of the organ when autumn comes upon the scene, and the reducing agencies of the protoplasm having been expended, the oxidising agencies of light and air enjoy unrestricted sway crowned by the golden and crimson glories of the autumn woodlands.

P. Q. Keegan.

Patterdale, near Penrith, Westmoreland.

IV

The Species, the Sex, and the Individual[1]

IN investigating the evolution of animal structure we have to consider not merely the features which distinguish species and groups of species from one another, but the difference between distinct types within the same species, and the changes which the individual passes through in the course of its life. The selection doctrine is held by its adherents to afford an explanation of the facts in all three of these divisions, but no one has hitherto pointed out the close similarity between the phenomena in the three cases, and attempted to prove that the principles he adopts are really applicable in detail to all of them. Examples of the differences of structure by which animals are classified are familiar to every one; the remarkable differences between male and female in the same species of bird, such as the peacock or pheasant, illustrate the second class of facts; and the differences between the tadpole and the frog, the caterpillar and the butterfly, illustrate the third.

Selection in nature can only mean the survival of an individual by virtue of some useful peculiarity in its structure, and selection as a theory of evolution can only be applicable to structural features which are of some use to the individual, enabling it either to obtain food or shelter, to escape enemies and natural dangers, or to reproduce its kind. Hence if the selection doctrine is true, everything in the structure of animals must be adapted to some useful end, must be an adaptation—or it must be the physiological corollary of an adaptive structure, must be correlated with some useful variation.

[1] With reference to this paper, Mr Cunningham has given us the following information, which we have verified. The paper was written at the beginning of 1897, and after some time was submitted to the Zoological Society, but not accepted, even for reading, on the ground that the Society did not usually publish papers of a theoretical and controversial character. The manuscript was then sent to the Linnean Society, where it was read on May 6th of the present year, and a brief description of it was published in the report of the meeting in the *Athenæum* and in *Nature*. But this Society also refused to publish the complete paper, the alleged reason being the pressure of other papers and illustrations. It is due to Mr Cunningham that these facts should be known, for on June 7th, 1898, there was read before the Zoological Society a paper by Mr L. W. Wiglesworth, containing conclusions as to sexual dimorphism very similar to those of the present paper. In particular, as the published abstracts show, the author maintained that secondary sexual characters in birds were due to the stimulation of parts through use, or external violence, or irritation.

So much for Mr Cunningham's title to priority. As for the refusal to publish his paper, we understand that the Zoological Society has equally refused that favour to Mr Wiglesworth, although he was more fortunate in having his views placed before a meeting, and published in abstract. There is a general feeling among those who hold views opposed to the current strictly Darwinian notions that they cannot get fair play from our learned societies. It is a pity that they should be able to adduce so many facts in support of this opinion, however erroneous the opinion itself may be.—Ed., Nat. Sc. I.

The conception of correlation means that, owing to something in the constitution of the animal, two or more features can only vary together in certain directions, for example it might be found that a dog could not have less hair without having smaller teeth, and the advantage of larger teeth might in such case be alleged as sufficient explanation of long or thick hair.

It is of course not necessary to the selection argument that all characters should be useful at the present day. They might have been evolved in a former period when they were useful under other conditions, and be still inherited after they have ceased to be useful.

However, the more we study the forms and structure of animals the more difficult we find it is to believe that all peculiarities by which the various species are classified are to be explained directly or indirectly by adaptation. We may study the question in reference to the characters distinguishing subdivisions of any degree, but it has been most discussed in reference to the peculiarities of species, and these can only be studied in actual examples.

The case with which I am most familiar is that of the flat-fishes.

The plaice, flounder, and dab, are three species of the same genus, whose habits and life-histories are fairly well known, and whose structural peculiarities have been minutely investigated. The dab is principally characterised by the presence of well-developed ctenoid or spinulated scales all over both sides of the body, and by a semicircular curve of the lateral line above the pectoral fin.

In the plaice we see conspicuous red spots; the scales are for the most part smooth, cycloid and reduced, the lateral line is straight, and the bony ridge behind the eyes is elevated into five tubercles.

In the flounder we find another condition of the scales: some are smooth and reduced in size as in the plaice, while others are enlarged and developed beyond the condition seen in the dab. Along the bases of the marginal fins there is a series of these enlarged scales, which form thorny tubercles; and there are others along the lateral line. Other peculiarities are the smooth ridge behind the eyes, and the small number of the fin-rays.

In other respects these fish are much alike. There are differences in their habits and life histories. The flounder lives in estuaries and rivers, only descending to the sea in the spawning season. The plaice and dab are almost invariably found together. The plaice feeds mostly on molluscs, the dab chiefly on crustaceans, worms and echinoderms. The young plaice congregate near shore, while young dabs are found at various depths. The plaice begins to spawn earlier in the year than the dab.

Now it is quite impossible, at any rate up to the present time, to find the slightest indication that the specific characters of these three species are useful in relation to these slight differences of life-history.

We cannot find that the rough tubercles of the flounder are useful to it because it lives in rivers or for any other reason, we know of no advantage which the plaice derives from its red spots, its smooth scales, or the bony tubercles on its head. Nor can we find any indication that these peculiarities are correlated with adaptive differences. The only adaptive difference at present clear is that the plaice has blunter teeth in its throat than the other species, and that these are suitable for crushing the shells of the bivalves on which the plaice feeds. But we know of no connection between this and the other characters. The theory then that these specific peculiarities are due to the natural selection of indefinite variations is unsupported by any evidence.

How then are we to explain such specific characters? It seems to me we are forced to regard them as the necessary consequences of growth and of the conditions of life. It is evident enough that differences of habit and the extension of a species into different regions will necessarily lead to its subdivision into groups between which little or no interbreeding takes place. The individuals which lived in estuaries would breed together and not with those that lived always in the sea. Thus any modification produced in the one group would remain confined to that group, and by interbreeding within that group would be kept approximately uniform. It is difficult in the present state of our knowledge to say how modifications of the kinds here in question are determined. There are indications that continuous modification takes place in successive generations without any direct stimulus that can be detected. We must remember that the development of the individual is the growth and multiplication of groups of differentiated cells. This development is controlled partly by heredity, partly by surrounding conditions, but the development of every part and every organ is to some extent independent. The facts indicate that a particular part or organ may for some unknown reason obtain increased nourishment and develop with increased vigour, or on the other hand may show diminished vigour, and that the change may be progressive in successive generations. It would seem that ultimately the effect must be due to external conditions acting upon the properties of living matter. But the action is evidently very indirect, and the processes involved are so complicated and recondite that at present we know nothing about them. We have indications of the influence of external conditions in the differences in the same species in different geographical areas. Thus the flounder in the Mediterranean has scarcely any tubercles, while in the Baltic and Arctic regions those structures are excessively developed, so that nearly the whole skin is covered with them. Whether this is due to the cold or not we do not know, but it is a fact that the plaice also shows greater roughness of

the scales in the north than in the south. The difficulty is that this effect of northern latitudes is not observed in the fishes of other families, and we have to face the problem how it is that the same change of conditions produces different effects in different cases. It is possible, however, to investigate the subject by analysis and experiment. The point on which I wish to insist here is that specific characters are intelligible when regarded as the necessary consequences of the conditions of life, while the supposition that they are of any use or significance in the struggle for existence is in a vast number of cases unsupported by any evidence.

Having thus indicated the reasons for rejecting the conclusion that all distinguishing characters are adaptive or advantageous, we may proceed to consider the origin of adaptations.

It may be truly said that no animal is without adaptations; it must be provided with some means by which the essentials of life are secured, but these means may be exceedingly simple or exceedingly complex. But there is another idea implied in the conception or adaptation, the idea of unity in diversity, of parts essentially similar being modified in different animals for different purposes, being adapted in many cases for purposes quite different from that which they originally served, as in the case of the fore-leg becoming in birds the wing. It is possible to trace such modifications and more or less disguised homologies without any reference to the doctrine of evolution. The principle of descent with modification gives the explanation of the phenomenon. The unity of plan is due to heredity; the divergence, to adaptation to changed conditions. But we must pursue the investigation further, and endeavour to discover how the modification is effected. It may be asked, since we admit that adaptation is such a prevalent phenomenon in the animal kingdom, even if it is not universal and exclusive, what other explanation of it is required than natural selection? In reply to this I would urge, in the first place, that natural selection implies and assumes the appearance of variations, of slight modifications by virtue of which certain individuals differ from their brethren and from their parents, in fact from all pre-existing individuals. The real explanation of evolution therefore lies in the explanation of individual variations. We admit that they occur, but how and why?

Darwin held that the use or disuse of organs and the direct action of conditions caused modifications of individuals in definite directions, and that these modifications were hereditary in some degree. Now, if once we admit this, selection becomes a secondary and subordinate factor. For if a new set of conditions or a change of habits caused a hereditary change of structure in all the individuals exposed to it, continuous modification would take place even if all the individuals generated survived, or if those which were

killed were taken at random without selection. Thus Darwin's system contained its own refutation within itself. A later school of evolutionists have maintained that the effects of habits or conditions on the individual are not inherited, and therefore not cumulative. According to this view, only those variations are hereditary which arise in the germ, in the internal constitution of the egg; such variations are supposed to be numerous and to take place in all possible directions, and natural selection is supposed to pick out from among them those which are advantageous and so accumulate them. I do not propose here to discuss the various theories of heredity. The question of the possibility of the transmission of acquired characters, or the determination of congenital modifications by the direct influence of conditions is a very important one, and has been much discussed. But I wish to draw attention to a mode of considering the subject which is generally neglected, namely, the inductive method. The doctrine of evolution is an induction from the facts of zoology; in my opinion conclusions concerning the method and the causes of evolution can also be obtained as inductions from a sufficiently wide survey of the phenomena.

Every one will admit without hesitation that all variations must be due to causes. But, according to the selectionists, hereditary variations have no primary and essential relation to the requirements of life. Such variations occur in all or many directions indefinitely, and they are so diverse that by the survival of a few individuals out of the many that are generated, the complicated adaptations which we know have been gradually produced. It is as though we conceived of a table being produced by the process of selecting from a large stock of pieces of wood of all shapes and sizes, those which were of the shape and size required, and joining them together; and not by the usual process of sawing and planing the various parts into shape out of a stock of planks all originally similar. Thus selection preserves and combines the variations which are most advantageous under the given conditions, but the relation between the structure and the outer world has no hereditary effect in moulding or shaping the structure. Romanes maintained with much truth that natural selection was a theory only of the origin of adaptations, and not necessarily of the origin of species, but it is further necessary to realise that it originates adaptations only in the sense of preserving and combining the variations or modifications which occur, and which happen to be advantageous. It may be said to combine only in the sense of causing different variations in the parents to be transmitted together to the offspring, and of allowing new variations to occur only in the individuals which have survived.

Now it is possible by actual observation to ascertain what evidence there is, that variations which might by natural selection

be combined into the adaptations we see, do occur apart from the special habits or conditions to which the adaptations are related. The variations that occur constantly in the form of individual differences, have been minutely investigated in the past few years by statistical methods, with the aid of the higher mathematics. The greater the difference, the more rarely it occurs; and occasionally striking abnormalities are observed, the character of which points to definite principles of symmetry and repetition in development. But it is not proved that, without change of conditions, variations occur which could by selection give rise to such special adaptations as abound in the animal kingdom. For example, the power of partial or complete flight by means of a membranous fold of skin, has been evolved in many independent cases in the vertebrate sub-kingdom, in the extinct pterodactyl reptiles, in bats, in flying foxes, flying squirrels, etc. But the variations in the condition of the skin and limbs in animals that do not fly or take long leaps through the air, are not such as to justify the belief that by the mere selection of the maxima among such variations, a membranous organ of flight could be evolved. To take another instance, there is a fish which has its eyes in a very remarkable condition. Spectacles for our own eyes, for human eyes, are sometimes made in which the upper half has a curvature different from that of the lower. The fish to which I refer, *Anableps*, does not wear spectacles, but actually has its eyes made in two parts, in the upper part of which the lens has a different curvature from that of the lower. The pupil is also divided into two by prolongations from the iris. This fish is in the habit of swimming at the surface with its eyes half out of the water, and the upper half of the eye is adapted for vision in air, the lower half for vision under water. Now, however various the individual variations in fishes' eyes, there is no evidence that variations which could by selection give rise to this curious condition, occur in other species of fish. It seems to me that we have no reason to suppose that the required variations ever occurred, until the ancestors of *Anableps* took to swimming with their eyes half out of the water. A similar argument applies to many other cases of special adaptation, and the logical conclusion is that the habits and conditions determined the modification.

On the other hand, it may be asked, what positive evidence have we that special habits or conditions do determine special modifications. The reply is, that we have abundant and admitted evidence as to the effect on the individual; and as hereditary modifications are, in many cases, of the same kind as these, the presumption is, that the effect on the individual has become hereditary. The question, however, of the origin and causes of adaptations cannot be considered apart from the phenomena of development and individual

metamorphosis to which I shall refer further on. At present I will pass on to the consideration of the second class of structural differences, those which distinguish constant forms within one species.

The commonest and most widely extended case of this is the existence of what are called secondary sexual characters, in other words the existence of structural differences between males and females in addition to the primary and essential differences in the generative organs. Darwin explained these differences by another kind of selective process, namely sexual selection. He pointed out that there is competition in courtship as well as in the struggle for existence, that the successful males are those which conquer their rivals by force, or which please the females best by their beauty of appearance or melodiousness of song. Now whether this is true or not, and there is certainly a great deal of truth in it, it is not sufficient to explain all the facts. In the first place it does not explain why the peculiarities of males do not begin to develop until the generative organs become functionally mature. If selection by the female were the principal factor, an earlier development would be an advantage. A male bird, for example, that already had its special plumage fully developed when he first became mature, would defeat those in which it had only just begun to develop, and consequently early development of the special plumage would soon become universal. The only way to meet this objection is to maintain that the young males find an advantage in being inconspicuous like the females, because they thereby escape their enemies, or that they obtain some other benefit in the struggle for existence by the retardation of the development of their secondary sexual characters. But when we study the matter without prejudice we find that the sexual peculiarities are associated with special habits and conditions, which do not come into force until maturity is attained, and we have reason to infer that the necessary modifications only occurred in connection with these habits and conditions.

As it is usually the male bird which is stronger, more active, and more adorned, some biologists have concluded that the whole constitution of the male is naturally more inclined to active physiological changes, that of the female more to simple vegetative growth. But there are plenty of cases to show that no secondary characters are invariably associated with the male sex rather than with the female; the evidence indicates that the characters are related to particular conditions and habits. In some species the usual differences between the sexes are reversed, the male is inconspicuous and resembles the young female, while the adult female has peculiar characters. In these species we find that the usual habits are also reversed.

Every one knows that there are enormous differences among different species in the degree of development of secondary sexual characters. In many cases there are no such characters, the males and females are similar; and in these cases not only are sexual differences wanting, but we do not find variations which if increased would lead to them. Secondary sexual characters in the plumage and in other structures are very conspicuous in the Class of birds, but there are numerous species of birds in which the male and female are scarcely distinguishable. It is an important fact that in the latter cases the birds are monogamous, pairing either for a whole season or for life, while birds in which the male plumage is in gorgeous contrast to that of the female are frequently polygamous. This fact has been emphasised by Darwin, but the significance of it in his view was that polygamy involved a relative excess in the number of males, so that those which obtain a plurality of wives have been selected from a large number, leaving a remnant which obtain no wives at all. I believe that the correct interpretation of the matter is very different.

It is well known that, in the most familiar cases of special plumage in male birds, this plumage is elaborately displayed in courtship in a definite manner peculiar to each species. As Darwin states, "Ornaments of all kinds, whether permanently or temporarily gained, are sedulously displayed by the males, and apparently serve to attract or fascinate the females." Now this display is an erection of the feathers by the muscles in the skin, and a movement, an agitation or vibration, of the feathers. We have every reason to believe that a mechanical movement of the feathers must irritate the papillae from which they are produced, and stimulate the proliferation of epidermis to which the growth of the feathers is due. Thus, if we consider only the increased size of the feathers apart from their colour or markings, we may conclude that the display and erection of certain feathers is the exciting cause of their excessive development in the males. This theory is supported at any rate by the fact that the degree of development of special plumage corresponds to the proportion of his life and activities which the male devotes to courtship. This may, in the present tendency of biological doctrines, be considered absurd, but it will be found, if the facts are examined, that it is literally and scientifically true. The pigeon, for example, pairs for life. He performs gestures of courtship it is true, but he also takes an equal share with the female in the duties of incubation and care of the young, and consequently his courtship consumes only a small portion of his time. A polygamous male bird on the other hand performs no part of the work of incubation or feeding the young, and in the breeding season spends a very large part of his time in displaying his plumage to his numerous partners.

In no family of birds are the males more gorgeous or more different from the females than in the Birds of Paradise. Darwin says in his treatise on Descent of Man and Sexual Selection that, according to Lesson, these birds are polygamous, but that Mr Wallace doubts it. The sexual selection therefore is to this extent less probable or less severe, but there is no doubt whatever about the difference of habits to which I attribute the difference of plumage between the sexes. In another passage in the same book Darwin writes: "With Birds of Paradise a dozen or more full-plumaged males congregate in a tree to hold a dancing party as it is called by the natives, and here they fly about, raise their wings, elevate their exquisite plumes, and make them vibrate, and the whole tree seems filled with waving plumes."

It may be objected that the mechanical stimulation which I have adduced as the cause of the hypertrophy of the feathers, will not explain their brilliant colouring or the beauty and symmetry of their markings. To which I would reply that stimulation of the growth probably causes also a more intense production of pigment; that symmetry of marking is a universal character in organic growth throughout the animal kingdom; and, thirdly, that very possibly the different qualities of the light to which males and females are exposed have something to do with the dull colours of the female which sits close with her young in obscure retreats, and the bright colours of the male which keeps more in the open.

I have already referred to the fact that in some species the relative characters of the sexes are reversed, and it is the females which are larger, more pugnacious, and more elaborately adorned than the males. Darwin, of course, attributes this to the reversal of sexual selection, but it seems to me more rational to hold that the differences are not merely selected but called into existence by the habits and conditions. In these cases the male alone performs the duties of incubation and nursing, and the female takes all the initiative in courtship. Here, as in the males in the usual case, the peculiarities of the female only begin to develop when she is approaching maturity, the young of both sexes being similar to the adult male. Species of *Turnix* in India and Australia are instances of this condition.

J. T. Cunningham.

1 Morrab Terrace,
Penzance.

(*To be continued.*)

V

The Delimitation of the Albian and Cenomanian in France

THE nomenclature of the English Cretaceous System is based upon the lithological differences exhibited by its members, the only division which from the beginning had a name of different origin being the Wealden. Such a basis of nomenclature is bad because lithological differences are local or provincial accidents.

French geologists have often expressed surprise at the conservatism of Englishmen in retaining a nomenclature which only perpetuates errors and cannot be made to express the true relations of the component parts of the Cretaceous System. They are quite right: it has perpetuated the error that the Gault as a whole is older than the Upper Greensand as a whole, and has prevented us from recognising long ago that they were to a large extent merely different lithological facies of one formation.

There can be no question that the distribution of species affords a better basis of grouping than the lithological characters of deposits. Put in this way it seems a truism, but it is nevertheless a fact that our existing system of nomenclature ignores this principle, and does actually separate deposits which ought to be grouped together; while it suggests a connection between 'Lower' and 'Upper' Greensand which has no existence in reality.

The French method of nomenclature is free from this reproach, and it has been preferred to our own by most other European nations. The French completely ignore lithological differences, and their subdivisions or stages include all deposits which yield a similar assemblage of fossils.

D'Orbigny says that his principal object in undertaking the "Paléontologie Française" was the application of palaeontology to the natural classification of the formations, and it is to him that the French owe their nomenclature of the Jurassic and Cretaceous systems. He found that of the Cretaceous system in dire confusion, but when he had examined 593 species of Cretaceous Cephalopoda and Gasteropoda he felt himself justified in dividing the whole system into five distinct stages, each containing a special fauna. This was in 1843, and, abandoning the lithological names which were then current in France, he proposed new names for his stages, taken from those of towns or districts where each stage was well developed and specially fossiliferous. These five stages were Senonian, Turonian, Albian, Aptian and Néocomian. In 1852 he added a sixth, having recognised that the group which he called Turonian in 1843, really comprised two stages with essentially differ-

ent faunas; consequently he restricted the name Turonian to the upper of these stages, and gave the name of Cenomanian to the lower.

Since 1852 these names have always been used by French geologists, but in spite of much careful work on the fossils, and in spite of the progress made in stratigraphical geology, the delimitation of these stages has never been satisfactorily settled. The restriction of the fossil species to the several stages did not prove so complete and exact as had originally been supposed. Many species were found to range from one stage to another, and where there is a gradual passage from stage to stage, there is of course scope for difference of opinion regarding the plane of separation. Moreover in the case of the Albian and Cenomanian there are special difficulties, for in the Aube whence was taken the type of the Albian, the fossiliferous beds corresponding to the Lower Gault are overlaid by a great series of almost unfossiliferous marls (representing Upper Gault), and the Cenomanian is neither well developed nor very fossiliferous. D'Orbigny, again, considered the Albian to be absent from the area taken as the type of the Cenomanian (la Sarthe), and this view is still held by many French geologists, though there are basement beds which some have regarded as older than the true Cenomanian.

When the stratigraphical succession in the north-east of France (Marne, Meuse and Ardennes) came to be better understood, it was found that between the beds which yielded typical Albian fossils and those in which only Cenomanian fossils occurred, thick deposits of sandy marl and fine-grained sandstone (gaize) came in, and that these beds contained a mixture of species, some being such as were originally regarded as Albian and others such as are generally confined to the Cenomanian. In these beds the prevalent *Ammonites* are *A. inflatus* (= *rostratus*) and *A. auritus*, and they came to be known as the zone of *A. inflatus*. Occasionally, however, they contain Ammonites of the species *A. mantelli*, *A. varians*, and *A. falcatus*, which are essentially Cenomanian forms.

D'Orbigny was not fully acquainted with the fauna of these beds, but he was aware that the Gaize de Montblainville contained such a mixture of species, and he nevertheless referred it to the Albian.[1] Further, he regarded the Gault of Wissant and of Folkestone as the equivalent of his Albian stage, and in his "Paléontologie Française," *Ammonites inflatus*, *A. varicosus*, *A. auritus*, and *A. mayorianus*, are given as Gault (*i.e.* Albian) fossils.

The Upper Gault with *Ammonites inflatus* continued to be regarded by most French writers as Albian, up to the year 1874, but Hébert in 1864, and Barrois in 1874, preferred to consider it as Lower Cenomanian, the latter giving as reasons (1) that the Gaize

[1] *Vide* his "Paléontologie et Géologie Stratigraphique." Tom. ii., p. 622, 1852.

de l'Argonne contains so many Cenomanian species, (2) that this zone overlaps the Lower Gault, and thus in his opinion separates itself from the latter. Later in 1876 Barrois expressly included the Upper Gault of Folkestone and Wissant in the Cenomanian. But he made no estimate of the number of species which united the zone of *Ammonites inflatus* at these places to the beds above, nor did either of them ever discuss the relative values of the different elements of the fauna of the *A. inflatus* zone.

The variation and discordance of opinion in France may be gathered from the change in the grouping of the zones in different editions of A. de Lapparent's well-known "Traité de Géologie." In his edition of 1885 the zone of *A. inflatus* is grouped as Albian, in that of 1892 it is placed in the Cenomanian. The latter method of grouping has been adopted by Mr G. Dollfus for the Service de la Carte géologique de la France. At present, therefore, most French geologists make a very small Albian and a very thick Cenomanian, while the line of separation between the two stages is drawn through the middle of the Gault of Wissant and through a perfectly continuous bed of sandy clay at Havre.

In England we have arrived at very different results; De Rance in 1868 and Price in 1874 showed that the Gault of Folkestone was separable into two divisions—a Lower Gault characterised by *Ammonites interruptus* and *A. lautus*, and an Upper Gault characterised by *A. varicosus* and *A. rostratus*. In 1876 Barrois published his excellent Researches on the Upper Cretaceous Series of England, and proved to us that the greater part of our Upper Greensand was the stratigraphical equivalent of the Upper Gault of Folkestone.

Subsequent investigations have led us to regard the combined Gault and Greensand as a single stage or natural group of beds, to abandon the names Gault and Greensand as denoting definite chronological divisions; they can only be regarded as descriptive of different lithological aspects or facies of the formation, and consequently as serviceable only on maps that are designed to exhibit such lithological variations. Hence we can recognise with Barrois a zone of *Ammonites rostratus* (= *inflatus*), but it is quite impossible for us to accept a classification which groups this zone with the Lower Chalk and separates it from the Lower Gault. We base our refusal on the very principle by which the French themselves profess to be guided, namely, on the faunistic relations of the several zones, and especially on the range and relative abundance of the different species of Cephalopoda.

Now a classification which appears to be the best and most natural expression of the facts in southern England can hardly be unnatural in the north of France, and thus it became clear that some study of the French sections from an English point of view was greatly needed. Such a study was made by Mr W. Hill in

1895, and the results were published in a joint paper with myself on the "Delimitation of the Cenomanian in England and France."[1] Careful and repeated examination of the fine coast section near Havre led us to dissent entirely from Professor Hébert's grouping and to agree with those French geologists who had placed the base of the Cenomanian above the local representative of the Gaize (or zone of *Ammonites inflatus*). We showed, in fact, that the series near Havre is obviously and naturally divisible into an Albian and a Cenomanian, which exactly correspond with the two English stages of (1) Gault-cum-Greensand, and (2) Lower Chalk.

Our descriptions and arguments did not however carry conviction to the mind of Mr G. Dollfus, who discussed the question in February last,[2] and maintained that our views were not in accordance with the palaeontological evidence. The July and August numbers of the same periodical contain a rejoinder to this attack, in which the friendly challenge was taken up and the palaeontological argument stated more fully than had previously been attempted either in England or France, with the advantage of being translated into French by my courteous opponent himself.

As the French nomenclature has been adopted in most European countries, it becomes a matter of international importance to decide what is to be connoted by the terms 'Albien' and 'Cénomanien.' I desire, therefore, to publish part of my reply to Mr Dollfus in its English version, and this (with a few small corrections) constitutes the remainder of the present article.

Referring to the section at Wissant Mr Dollfus remarks: "If Mr Jukes-Browne wishes us to place the line of separation above the clay with *Ammonites inflatus*, far from making us take a step forward, he would lead us backward; his opinion is that of a period which we out-grew in France twenty-five years ago." That depends on the point of view; I think that twenty-five years ago the geologists of France took a path which deviated from the right road; it is quite true that I seek to lead them back from this wrong path, and I propose that we should walk together along the straight highway of progress. That is my hope, but I know that I have first of all to essay the difficult task of persuading my *confrères* that the path they took was a wrong one.

In the first place let us consider the Cenomanian of the typical area near Le Mans. How was this stage established? D'Orbigny did not go into stratigraphical details, but he studied the fossils which had been found in the beds near Le Mans, and he saw that the fauna as a whole was different from that of the Albian of Dienville and the Gault of Wissant. D'Orbigny, it is true, thought that there was only

[1] *Quart. Journ. Geol. Soc.*, vol. lii., p. 99 (1896).
[2] *Feuille des Jeunes Naturalistes* for February 1898, No. 328.

one fauna at Le Mans, but then he also thought the Albian was not present at Havre, where it is now admitted to exist. In the same way it may be necessary to admit the existence of Albian at Le Mans.

Now what does present itself at Le Mans? I will quote the words of Mr Dollfus: "At Le Mans the base of the Cenomanian consists of sands containing *Ostrea vesiculosa*, *Nautilus subelegans*, *Pecten asper*, *Ammonites inflatus*, etc. These sands rest on Oxfordian beds with *Rhynchonella varians*, without the interposition of any beds belonging to the Lower Cretaceous; this lower limit is therefore very clearly marked, for it is based on a considerable stratigraphical break." But what kind of break is here? There is no break in the Cretaceous series, only an incomplete condition, from the absence of everything below a certain horizon in the series.

It is this very break or hiatus which is a source of difficulty, for, if the succession in the Sarthe had been complete, I do not think this discussion about the base of the Cenomanian would ever have arisen. French geologists have hitherto taken for granted that everything at Le Mans must be Cenomanian down to the local base. It is this to which I object: this assertion must be proved, not taken for granted. My position is this, that the delimitation of the Cenomanian cannot logically or properly be settled in La Sarthe. Its upper limit can be determined there because both Cenomanian and Turonian are fully developed; but its lower limit cannot be determined there; this must be done in some other region where both Albian and Cenomanian are fully represented.

I do not ask my French *confrères* to come to England for the decision of this question, nor do I ask them to accept a new name: I recognise that it is primarily a French business, and I will accept the evidence of the French strata. But I do say this, that the matter must be judged by the evidence of the fossils found in the region which is selected for trial, and that the fossils of La Sarthe must be left out of the account while the comparison is being made between the fauna of the zone of *A. inflatus* with the faunas of the beds above and below it.

Where then in France should this comparison be made? Not at Havre where the Albian is little developed; not in the Pays de Bray where fossils are rare in the Gaize. It is to the east of France that Mr Dollfus himself appeals on this point, maintaining that the fauna of the zone of *A. inflatus* at Wissant and in the Gaize de l'Argonne is so different from that of the zone of *A. interruptus*, that it must be grouped with Cenomanian, not with the Albian. I accept this test, but I do not come to the same conclusion.

Let us take first the Gaize de l'Argonne. Mr Dollfus says that the list published by Mr Barrois shows that 51 species are Albian (Gault Inférieur) and that 70 are Cenomanian; but the latter figure is

his, not that of Mr Barrois, who only says "un assez grand nombre d'espèces cénomaniennes" (a considerable number of Cenomanian species). I do not understand how Mr Dollfus arrives at the 70 unless he brought the fossils of the Sarthe into account. This, I contend, should not be done; those only should be marked which occur above the Gaize in the east of France; *i.e.* those recorded by Mr Barrois in his "Terrain Crétacé des Ardennes," in the beds which lie between the Gaize and the Turonian. I have done this and find that only 48 range upward. The numbers 51 and 48 are so near that it is clear the Gaize de l'Argonne will not decide the question.

We come next to the Upper Gault of Wissant, as to which Mr Dollfus says that d'Orbigny was wrong in referring it to the Albian. I cannot find that any French geologist has published a list of the fauna of the Upper Gault at Wissant. Mr Barrois tells me that he does not know of any such list, and in classing the upper part of the Wissant clay as Cenomanian, he seems to have relied on the proofs of its correspondence with the Gaize de l'Argonne, and on the greater extension or overlap of the zone of *A. inflatus*.

A list of the Wissant fauna has, however, been published in England by Mr F. G. H. Price, who sent the fossil-collector, J. Griffiths of Folkestone, over to Wissant for the express purpose of collecting separately from the Lower and Upper Gault of that place. The results were embodied in a small treatise on the Gault published by him in 1879, but the Wissant lists have never been printed separately. In this connection it will be useful to give a list of the fossils found by Griffiths in the Upper Gault of Wissant, indicating at the same time how many occur also in the Lower Gault of that place and how many range up into the beds above.

	Lower Gault.	Cenomanian.
Ammonites auritus, Sow.	x	
„ *cristatus*, de Luc.		
„ *delaruei*, d'Orb.	x	
„ *latidorsatus*, Mich.	x	
„ *lautus*, Sow.	x	
„ *mantelli* (?) Sow.		x
„ *rostratus*, Sow (inflatus).		
„ *splendens*, Sow.	x	
„ *tuberculatus*, Sow.	x	
„ *varicosus*, Sow.		
Ancyloceras spinigerum.	x	
Belemnites minimus, List.	x	
Hamites flexuosus, d'Orb.	x	
„ *elegans*, d'Orb.	x	
„ *intermedius*, Sow.	x	
„ *virgulatus*, d'Orb.		
Nautilus clementinus, d'Orb.	x	
Aporrhais parkinsoni, Mant.	x	
Dentalium decussatum, Sow.	x	
Scala dupiniana, d'Orb.	x	
Solarium conoideum, Sow.	x	

	Lower Gault.	Cenomanian.
Solarium dentatum, d'Orb.	x	?
Anomia sp.		
Arca carinata, Sow.	x	x
„ *glabra*, Sow.	x	x
Cardita tenuicosta, Sow.	x	x
Corbula elegans, Sow.		
„ *socialis*, d'Orb.	x	
Lima parallela, Sow.	x	
Inoceramus concentricus, Sow.	x	
„ *sulcatus*, Sow.		
Nucula ovata, Sow.	x	
„ *pectinata*, Sow.	x	x
Ostrea arduennensis, d'Orb.	x	
Pecten raulinianus, d'Orb.		
Plicatula pectinoides, Sow.	x	x
Terebratula biplicata, Sow.		x
Serpula articulata, Sow.		
Cidaris gaultina, Forbes.		
	27	7 or 8

In the above list there are 38 named species, and of these no fewer than 27 occur in the Lower Gault of the same place, which is in the proportion of 71 per cent., while only 7 or 8 (about 20 per cent.) range into the Cenomanian beds above. D'Orbigny, it therefore appears, was perfectly right in classing the Upper Gault as Albian and there is no necessity for revising the lists of the Albian fossils given by him in his "Prodrome." It is Messrs Barrois and Dollfus who have made a mistake by classing the Upper Gault as Cenomanian without a sufficient study of its fauna.

Turning now to England, let us choose localities which correspond most nearly with Wissant and with l'Argonne; it is generally admitted that the Gault of Folkestone is an expansion of that of Wissant, and it will not be denied that the Gaize of Devizes resembles that of Argonne.

I have made a list of the fossils of the Upper Gault of Folkestone, basing it on that of Mr Price (*Quart. Journ. Geol. Soc., Lond.*, 1874) supplemented by his later record in 1879. Selecting the Mollusca and neglecting other fossils, I find that the Upper Gault has yielded 103 named species; of this number 54 occur also in the Lower Gault, while only 29 range upward into the Chloritic Marl and Lower Chalk (Cenomanian) of Kent. Here, therefore, the zone in question has a much more decided affinity with the beds below than with those above.

Passing to the Gaize of Devizes my lists are not quite complete, but they include 92 species of Mollusca, and no less than 44 of these do not range out of the zone of *Am. rostratus* (or *inflatus*), but 36 occur in the Lower Gault of Wiltshire and Folkestone, while only 18 range into the Warminster sand, and 20 into the Lower

Chalk (including the zone of *Stauronema*). Here also the true relations of the fauna are quite clear.

In this study of the fauna of the English zone of *Am. rostratus* I have excluded Echinoderms, because their evidence is not of first-rate value; in support of this opinion, I may quote that of Dr J. W. Gregory of the British Museum, who has remarked that, "Echinids are rather a clue to the conditions of formation of deposits than evidence of their exact contemporaneity in age." Thus it may be quite true, as Mr Lambert declares, that the affinities of the Echinoidea found in the Gaize of Havre are Cenomanian, but their evidence cannot weigh against that of the Cephalopoda which are most clearly not Cenomanian.

Mr Dollfus quite omits to notice that I have appealed to the Cephalopoda as affording the best criterion of the affinities of the fauna of these beds. Let us see what this criterion proves.

From the Upper Gault of Folkestone Messrs Price and De Rance have recorded 31 species of Cephalopoda, and of these 12 range down into the Lower Gault, and only 4 range into beds above. Similarly in the Gaize of Devizes there are 20 Cephalopods, of which number 7 occur in Lower Gault and 4 range into higher beds. Both at Folkestone and Devizes *Ammonites rostratus* and *A. varicosus* are restricted to the Gaize and Upper Gault, but in the counties of Buckingham and Bedford, I have myself found both species in the Lower Gault. In two cases they were in company with *A. lautus* and *A. splendens* about 30 feet from the base of the Gault which is there 200 feet thick; in a third case they occurred with *A. interruptus* quite near the base of the Gault. With respect to the upward range of *A. rostratus*, it passes upward into the green glauconite sands above the Gaize, both in Wiltshire and Dorset, and where the Chert Beds are absent, I have found it within six or seven feet (two metres) of the top of the Upper Greensand. No Ammonites have yet been found in the Chert Beds, but *A. rostratus* has never been found above them.

Next let us consider the true Cenomanian group of Cephalopoda:—*Ammonites varians, falcatus, mantelli, navicularis, rotomagensis, Scaphites aequalis, Turrilites costatus, tuberculatus.* No one who has collected along the south coast of England or at Wissant, or at Havre, could doubt where these species first set in as common fossils; they come in with *Stauronema carteri* and *A. laticlavius* in what is generally called the Chloritic Marl.

It is only occasionally and locally that any of them occur below this horizon, but so far as my experience goes not one of them ranges further than six feet below it. *A. varians, falcatus* and *navicularis* are not uncommon at the top of the Greensand between Warminster and Maiden Bradley, but are not associated with A. rostratus; the bed in which they occur is evidently a passage bed from Greensand to Chloritic Marl.

I only know of two cases where *A. varians* and *A. rostratus* are said to be associated: one is in the highest bed of the Greensand in North Dorset where both are rare. The other is the record by Mr Price of *A. varians* from his bed XI. at Folkestone, but the *varians* was a doubtful specimen and no other has since been found. Mr Munier-Chalmas states that *A. varians* occurs with *A. rostratus* in the East of France, and doubtless it does occur occasionally at the summit of the *A. rostratus* zone. Such occurrences are not rare near the junction of two stages, and they only prove that there is no hard and fast line separating one fauna from another. It is generally conceded that in the delimitation of zones or stages, we must be guided by the abundance of certain characteristic species not by the mere occurrence of one or two of them. Consequently those who argue that the zone of *A. rostratus* must be Cénomanien because *A. varians* sometimes occurs near the top of it are reversing the rule, and seek to establish a precedent which would destroy our principles of classification.

Mr Dollfus has said "it is an illusion to think that we can ever possess a perfect classification which would satisfy geologists of all countries." That is quite true, but it is not an illusion to believe that we can frame a classification which will suffice for one stratigraphical province or basin of deposition, such as that of England or Northern France.

I do not seek to upset the French nomenclature, on the contrary I recognise that the French will accept no other nomenclature. Endeavours have been made to employ it in England, but at present that is impossible. D'Orbigny's names have been so wrested from their original application by the modern French geologists that many English geologists think that these names no longer possess any fixity of meaning; consequently they oppose the adoption of them. There are of course some who see that the alterations are merely personal views of grouping, and that the faunistic differences on which the names were founded remain the same. So far as the divisions of the Chalk are concerned, we may eventually be able to employ the French names, and the time will be hastened if our French *confrères* will restrict the Cenomanian to its proper limits; but with respect to the English Gault and Greensand I see no alternative but to propose a new name.

I hope at any rate to have made it clear that I do not advocate "a return to antiquated classifications based mainly on mineralogical facies." On the contrary I believe that Mr Dollfus and I are in perfect accord in regard to the principles of classification. We both consider palaeontology and stratigraphy to be the true guides, and we only differ because we interpret the teaching of these guides in different ways.

A. J. Jukes-Browne.

Etruria, Torquay.

SOME NEW BOOKS

Welwitsch's African Plants

Catalogue of the African Plants collected by Dr Friedrich Welwitsch in 1853-61. Dicotyledons, Part II., Combretaceae to Rubiaceae. By W. P. Hiern, British Museum (Natural History). 8vo, pp. 337-510. London: Printed by order of the Trustees. 1898. Price, 4s.

The number of those who have braved the dangers and discomforts attendant upon botanical journeys in distant countries is continually on the increase. Such men, to cite only a few names, as Robert Brown, Von Martins, Spruce, Weddell, Hooker, Beccari, have highly distinguished themselves by honourable labour in this department of science, and their services have been gratefully recognised by the world at large. But among them all, none is more worthy of recognition than Dr Friedrich Welwitsch, who, for eight years, explored the at that time all but unknown Portuguese possessions in the south west of Africa. Welwitsch's great merit resides in the thoroughness with which he set himself to perform his allotted task. It is one thing to pass rapidly through a country, plucking specimens when opportunity offers, as a member of an expedition protected by all the resources of civilisation against the many unpleasantnesses which would otherwise have to be encountered. Very different must it be when the solitary traveller has to rely upon his own devices; when year by year he struggles on against the scorching heat of the tropics, against the swarming insect life, whose only object seems to be the reduction of man to the lowest ebb of wretchedness, against the vicissitudes of the seasons with the inevitable diseases lurking in their train, against the ever-present danger from noxious animals and still more noxious members of the human race. All this Dr Welwitsch did, and the result is seen in the truly splendid additions to our knowledge of the tropical African flora which we owe to his instrumentality. No one could possibly have turned his opportunities to better account; and when we search the record of achievement in this branch of knowledge, we fail to remember one explorer who, in the matter of scrupulous care in the selection of his specimens, and ungrudging toil and sagacity in the writing of the notes to accompany them, can be mentioned as Welwitsch's equal.

Unfortunately the great explorer died before he was able to give to the world the full result of his unparalleled efforts; but a fine set of the plants, equal to all intents and purposes to the first set now at Lisbon, was happily secured by the Trustees of the British Museum. Owing to pressure of work, these plants for some years remained undescribed; meanwhile sets of inferior value were distributed from Lisbon to various herbaria, and by these means descriptions of Welwitsch's novelties have exercised the pens of various botanists from time to time. But desultory work of this kind, however useful it may be, is scarcely a worthy way of dealing with the subject: it is therefore a matter for

unalloyed satisfaction that Mr Hiern has stepped into the breach by undertaking a full elucidation of Welwitsch's collections. In the second part of the memoir devoted to this object, the one we are here noticing, the interest aroused by the appearance of the first part is fully maintained, as is also the high reputation of the author. Mr Hiern is well known for the painstaking accuracy of all his work; he is, also, a man whom, in any case when divergency of view is admissible, one would much rather have on one's own side than as an opponent. Moreover, the excellent judgment he displays in making full use of the explorer's notes, gives him an extra claim on our gratitude. True, we have one objection to make in respect of this, an objection which may perhaps seem odd as coming from an Englishman. We think that too much of the memoir is written in our own language. After all, Welwitsch was a German, and the country he so thoroughly explored is a possession of the Portuguese crown. Moreover, a large number of those to whom the work appeals are foreigners. If, therefore, our objection be ruled invalid so far as concerns the notes themselves, we certainly see no good reason for departing from the time-honoured practice of describing in Latin at least the salient features of a new plant. By the use of Latin a person of any nationality at once knows what an author is driving at, and we can only hope that our Russian and Hungarian brethren will not resort to reprisals; otherwise troublous times are in prospect.

Combretaceae occupy the place of honour in the present part; thence we pass on to Myrtaceae, and so through the remaining calycifloral orders to the first order of the Gamopetalae, the Rubiaceae, and with this the part closes. We much like the pithy introductions to the principal orders, wherein geographical data, economic uses, and so forth are skilfully detailed. As regards nomenclature, we are glad to see that Mr Hiern does not lend himself to the extreme views prevalent in some quarters. To most of the changes he introduces no one who admits the advisability of change at all can possibly object. But we must confess that had the original disturber of nomenclature come to us for advice regarding the use to be made of his portentous knowledge, we should have felt disposed to answer in the words of the lady at the close of Mr Austin Dobson's delightful lovers' quarrel:—

> "I'd say no more about it
> If I were you."

But a great deal has been said about it, and much more written, so that one begins to think the best way out of the difficulty to be the adoption of the change as soon as possible. We grieve, though, to see an old and familiar name like *Psychotria* disappear; a change involving scores of species, and a large addition to the list of synonyms. And why does Mr Hiern refrain from attaching his name to species now for the first time ranged under some new denomination? Thus he prints *Myrstiphyllum cristatum* (the new name for the old *Psychotria cristata*, Hiern) without appending any authority, and so on throughout the work.

Oversights are very rare; but such a sentence as this—"Africa is but little favoured with the natural occurrence of Myrtaceae" is far from elegant, though it is only fair to say that we have found no

similar case of faulty diction. On p. 371 *Rotala verticillata* is printed for *R. verticillaris*, and "near genus" (p. 388) should obviously be "new genus." But errors like these are unimportant and, indeed, almost unavoidable. The main point to recognise is that we have here the second instalment of a work which shows excellent promise of proving creditable alike to the author and to the naturalist of whose 'grit' and sagacity it bears such unequivocal signs.

For Amateur Gardeners

Garden-Making: Suggestions for the use of home grounds. By L. H. Bailey, aided by L. R. Taft, F. A. Waugh and Ernest Walker. 8vo, viii+418 pp. New York: The Macmillan Co. 1898. Price 4s. net.

The Pruning-Book: A monograph of the Pruning and Training of Plants as applied to American conditions. By L. H. Bailey. 8vo, xii+538 pp. New York: The Macmillan Co. 1898. Price 5s. net.

Although these two charming little books are written primarily for American readers, they should be none the less welcome in this country. There are many things that we can learn from the ingenuity of our Transatlantic cousins, and under the guidance of Professor Bailey, we are sure to do so in the most pleasant manner possible. The number of books that this genial author contrives to publish during a single year is a marvel in itself, even making allowance for the help of various colleagues; but what is even more remarkable is the verve with which each is written. Professor Bailey, it is clear, enjoys writing his books, and that is why we all enjoy reading them. The illustrations, too, are always good and appropriate.

The book on Garden-making is original, with quaint fancies here and there, but practical withal. Its opening paragraph is one of the most fascinating introductions to a fascinating subject that we remember. Let us quote some sentences. "Every family can have a garden . . . one plant in a tin may be a more helpful and inspiring garden to some mind than a whole acre of lawn and flowers may be to another. The satisfaction of a garden does not depend upon the area, nor, happily, upon the cost or rarity of the plants. It depends upon the temper of the person. One must first seek to love plants and nature, and then to cultivate that happy peace of mind which is satisfied with little. If plants grow and thrive, he should be happy; and if the plants which thrive chance not to be the ones he planted, they are plants nevertheless, and nature is satisfied with them. . . . We are happier when we love the things which grow because they must. A patch of lusty pigweeds, growing and crowding in luxuriant abandon, may be a better and more worthy object of affection than a bed of coleuses in which every spark of life and spirit and individuality has been sheared out and suppressed. The man who worries morning and night about the dandelions in the lawn will find great relief in loving the dandelions. Each blossom is worth more than a gold coin as it shimmers in the exuberant sunlight of the growing spring, and attracts the bees to its blossom. Little children love the dandelions: why may not we? Love the things nearest at hand, and love intensely. If I were to write a motto over the gate of a garden, I should choose the remark which Socrates made

as he saw the luxuries in the market: 'How much there is in the world that I do not want!'"

The Pruning-book is eminently practical, but in the right way. All the advice is based on a body of solid principles, and these are explained by reference to the life-histories of various typical branches. Anyone who has mastered the instances given by Professor Bailey should be able to work out for himself the correct mode of pruning any unfamiliar tree. A hundred and forty pages are devoted to American viticulture, but this need not be grudged by us, as the rest is well worth the money.

Birds near Sydney

The Birds of the County of Cumberland. By Alfred J. North, C.M.Z.S., Australian Museum, Sydney. Reprinted from the Handbook of Sydney and the County of Cumberland. Melbourne: George Robertson & Co. 8vo, pp. 116.

Mr North has produced a useful pamphlet for local ornithologists. His list of twenty-one species does not include any but well-known Australian birds; but it has been compiled with manifest care, and should enable any visitor to ascertain what species he might hope to study during a few months' residence within the prescribed topographical limits. It is satisfactory to learn that the Lyre-bird (*Menura superba*) still frequents certain spots in the mountain ranges, and that the Black Swan (*Chenopsis atrata*) is still common in most of the inlets along the coast. Our information concerning the Freckled Duck (*Stictonetta naevosa*) is already so meagre that we wish that Mr North could have supplemented his reference to the occurrence of this bird in New South Wales in 1897, with a few fresh facts as to its life history. The families of *Timeliidae* and *Meliphagidae* include many of the most characteristic Aves of this district; but the Order *Psittaci* is also much in evidence. H. A. Macpherson.

Alexander Goodman More

Life and Letters of Alexander Goodman More, with Selections from his Zoological and Botanical Writings. Edited by C. B. Moffat, B.A., with a preface by Frances M. More. 8vo, pp. xii. 642. Dublin: Hodges, Figgis & Co., 1898.

The late Mr A. G. More possessed such a charming personality that there could be no doubt as to the wisdom of republishing his correspondence with the late Charles Darwin, Professor Newton, and other well-known zoologists. But the present volume does far more than this. A large portion of the text is occupied by pleasant letters and extracts from the diaries of the late Curator of the Dublin Museum; but more than two hundred pages are devoted to the reproduction of Mr More's essays and papers on Irish botany and zoology. Two of these articles are of wider interest than the rest, viz., those on the "Distribution of Birds in Great Britain during the Nesting Season," and the "Geographical Distribution of Butterflies in Great Britain." The former may indeed be considered classical, and proved of the utmost value to later workers. But A. G. More expended his greatest efforts in advancing the extension of Irish natural history. The impetus which his personal influence lent to the original researches of such Irish naturalists as Barrington and Barrett-Hamilton can best

be ascertained by a careful study of these pages. The most valuable piece of work which poor More accomplished during his later years was undoubtedly the official list of Irish Birds; the second revised edition of which appeared in December 1889, and is now reproduced, with footnotes by the Editor and Dr Scharff, which bring it fairly up to date. The statement (p. 368) that the specimen of *Turdus migratorius* procured in Ireland in 1891 was the first obtained in Europe is, of course, a mistake, at least five examples having been proved to have strayed across the Atlantic previously, one of the number having occurred in England in 1876; but this is a venial fault. The Shearwater, catalogued as a specimen of *Puffinus obscurus*, obtained off the Kerry coast in 1853, has recently proved to be *Puffinus assimilis*; but in other respects this catalogue of Irish birds is invaluable for reference purposes. The smaller papers on Irish zoology cover a variety of ground, and the index has been carefully compiled. H. A. M.

Some Jamaican Jelly-fish

The Cubomedusae. By Franklin S. Conant. Memorial Vol., with Biographical Sketch. *Mem. Biol. Lab., Johns Hopkins Univ.* IV. Part 1, xvi+62 pp., viii. pls. and frontispiece. Baltimore, 1898.

The friends of the late Dr Conant, with the aid of the Johns Hopkins University, have printed in the form of a memorial volume his dissertation on the anatomy of the Cubomedusae.

There is no group of the Coelentera which needed the careful investigation of a clever student more than the one which Conant chose for the subject of his thesis for the degree of Doctor of Philosophy at the John Hopkins University.

The Cubomedusae present so many features of exceptional interest that zoologists have felt very keenly that a reinvestigation of their anatomy and a study of their development were among the most important pieces of work yet to be done in the group of the Coelentera. The investigations of Claus and Haeckel, who were able to study preserved material only, are necessarily incomplete and unsatisfactory, and Conant seized the opportunity which the discovery of large numbers of the living medusae on the coast of Jamaica gave him of reinvestigating the whole subject. There can be no doubt in the minds of those who read the volume which records the results of his labours, that this contribution to science is a solid and valuable one. His descriptive writing is remarkably lucid, his reasoning clear, and at the same time cautious, and the numerous illustrations to the memoir are admirable. With such impressions framing themselves as we read the pages, there comes the feeling that in Conant we have lost a zoologist who had every prospect before him of a brilliant career in the scientific world. His patient and noble devotion to the cause he had at heart demands our admiration, and calls out our sympathy for his friends and fellow-workers in America who mourn his untimely death.

The species that Conant had to work upon were *Charybdea xaymacana* and *Tripedalia cystophora*, both of which were found in Kingston Harbour, Jamaica, the latter being the sole representative of the new family Tripedalidae. The habit of these two species is

not their least remarkable feature. The Cubomedusae were generally considered to be deep-sea forms, but both these species are found at the surface and near the shore. *Tripedalia* occurs—very locally—in water that is not only very shallow but discoloured with organic matter, with a bottom of black mud. It is very unfortunate that Conant was unable to complete his embryological investigations, but the following note on p. 23 concerning *Tripedalia* is of very great interest. "The embryos were thrown out in the Aquaria as free-swimming planulae, which settled down on the bottom and sides of the glass in a day or two, and quickly developed into small hydras with four tentacles. . . . In this condition they lived for three weeks without essential change, and they were still giving no promise of further development when the laboratory broke up and the jars had to be emptied."

Tripedalia is the smallest form of the Cubomedusae, the height of the bell in the largest individuals being only 8 or 9 mm., whereas of the twenty species mentioned by Haeckel only two are less than 20 mm. in height. The generic name is given to it on account of the prominent feature of the arrangement of the tentacles in groups of three, with separate pedalia. Further details of the anatomy of this new genus are given in the text.

The greater part of the dissertation is occupied by an admirably lucid account of the anatomy of *Charybdea xaymacana*, containing several new points of interest; but perhaps the most important part is the detailed description of the vascular lamellae and the nervous system of the Cubomedusae at the end of the volume. It will be noted with some interest that Schewiakoff's account of the histology of the eyes was not confirmed in all details. It will be remembered that Schewiakoff recognised in the retina two kinds of cells which he named visual cells and pigment cells respectively. This Conant was unable to do; in fact he found considerable evidence against the two types of retinal cells, and he found that the long pigment streaks are parts of retinal cells continued into processes like Schewiakoff's visual cells.

There is one point in the Memoir which needs criticism in case it is copied by others who succeed Dr Conant in the literature of Medusae. The use of the word 'gelatine' in the sense in which the word 'mesogloea' is used in this country is not justifiable. Mr Sedgwick in his recently published text-book objects to the word 'mesogloea' because "it suggests an ectogloea and entogloea which do not exist," and says that when the supporting lamella is thick and bulky it is simply called the 'jelly.' There is no very serious objection to the use of the word jelly in this manner, because it implies nothing more than a substance of jelly-like consistency; but the word 'gelatine' implies a definite chemical character, and all the evidence we have at present tends to prove that the mesogloea of Coelentera is not gelatine but mucin. It is therefore to be hoped that the word 'gelatine' in the sense in which it was used by Dr Conant will be dropped and the word mesogloea take its place. Mr Sedgwick's objection to the word cannot be considered a very serious one, for it would be equally applicable to the word mesentery, which he uses throughout his book.

SYDNEY J. HICKSON.

The Skeleton of Maryland

Maryland Geological Survey. Volume One. By Wm. Bullock Clark, State Geologist, E. B. Matthews, and L. A. Bauer. 8vo, 540 pp. and 17 plates.

With its first volume of Reports, the Geological Survey of Maryland makes an imposing, attractive, and generally successful start. This Survey has the benefit, inestimable in America, of being removed from the immediate control of politicians, since it is under the direction of a commission "composed of the Governor, the Comptroller, the President of the Johns Hopkins University, and the President of the Maryland Agricultural College." We may therefore expect the fulfilment of the promise that other volumes will follow, dealing with the mining, geology, palaeontology, mineralogy, forestry, agricultural physics, and so forth.

The present volume opens with an Introduction by Professor Clark, who relates the history of the establishment of the Survey, describes its plan of operations, and then gives an account of the progress of investigation concerning the physical features and natural resources of Maryland. This is followed by an outline of present knowledge of the physical features of the State, embracing an account of the physiography, geology, and mineral resources. This also is from the pen of Mr Clark; it occupies 87 pages, and is illustrated by a hypsometric and a geological map and several photographic plates of scenery. A useful appendix to this is a bibliography and cartography of Maryland, with special reference to its geology, by Dr E. B. Matthews, who was assisted in the compilation by all the members of the Survey. The volume concludes with the First Report upon Magnetic work in Maryland, by Dr L. A. Bauer, who has been conducting this division of the work of the Survey.

The State of Maryland forms part of the eastern border region which stretches from the Atlantic coast to the crest of the Alleghanies, and from its central situation affords, perhaps, the most characteristic section of this broad belt. In Maryland, as elsewhere, this part of the continent is divided into three physiographic areas: the Coastal Plain, the Piedmont Plateau, and the Appalachian Region. The Coastal Plain includes rather over one half the land area of the State (nearly 5000 square miles), and attains in places a width of 100 miles. By the great Chesapeake Bay, running north and south, it is divided into the very low-lying Eastern Maryland, and a higher western trait usually called Southern Maryland. The Piedmont Plateau occupies somewhat over one quarter of the land area of the State, being about 65 miles wide in the north, and gradually narrowing to 40 miles in the south. It is divided by Parr's Ridge into an eastern and western division. The former has a diversified topography due to varied crystalline rocks with complicated structure; in it are broad, fertile limestone valleys running in various directions. The western division includes the broad limestone valley of the Monocacy, on which is the town of Frederick, and near the mouth of which is Sugarloaf Mountain rising rapidly to a height of 1250 feet. The Appalachian Region extends from the Piedmont Plateau to the western limits of the State; it comprises about 2000 square miles, and attains a maximum width of 115 miles in the northern part of Mary-

land. It consists of parallel mountain ranges, with deep valleys, which are cut, nearly at right angles, throughout much of the distance, by the Potomac river. It is divided into three districts, based upon clearly defined geological differences; these are an eastern (Blue Ridge and Great Valley), a central (Appalachian Mountains proper), and a western (Alleghany Mountains).

Within the relatively small limits of the State of Maryland there is hardly an important geological epoch that is not represented, the most important omissions being in the Jurassic, with the possible exception of its later portion. The seven regions just indicated are each composed of a distinct series of geological formations. The beds of the Coastal Plain are nearly horizontal, still there is a predominance of the latter Cainozoic formations in the eastern division, and of Mesozoic and early Cainozoic rocks in the western. East of Parr's Ridge, in the Piedmont Plateau, is a sequence of highly crystalline rocks, largely igneous, which represent the remains of a vast Archaean Continent, whose detritus furnished the Palaeozoic sediments. West of Parr's Ridge are greatly folded and metamorphosed, but less crystalline, beds of early Palaeozoic time; the Frederick valley, above alluded to, lies in blue Palaeozoic limestone in part overlaid by the red sandstone and shale of Mesozoic age. In the Appalachian region, Blue Ridge and Great Valley are in Cambrian and Lower Silurian (= Ordovician) rocks, in places so eroded as to expose the Archaean floor; the Appalachians are built of Upper Silurian and Devonian strata; the Alleghanies are composed of more gently folded Devonian and Carboniferous deposits, carrying the valuable coal seams of the Cumberland basin.

It is thus clear that ample opportunity is afforded to Professor Clark and his assistants of producing a series of memoirs of varied and profound geological interest. The present volume forms an excellent basis, and augurs well for the future.

Down and Antrim for the Holidays

Belfast and County Down Railway Company. Official Guide to County Down and the Mourne Mountains. By R. Lloyd Praeger, with seventy [reproductions of] photographs of Scenery by R. Welch, Belfast, maps, and other illustrations. 8vo, xvi + 232 pp. Belfast: Marcus Ward & Co. 1898. One Shilling.

Official Guide to the Belfast and Northern Counties Railway, the Giant's Causeway, and the Antrim Coast. Maps and illustrations. 8vo, viii + 172 pp. Belfast: W. & G. Baird. 1898. Sixpence.

These are two excellent handbooks for the tourist in Counties Down and Antrim. Of late years a very great improvement has taken place in the accommodation for visitors to the North-east and the West of Ireland especially, and travel there is as comfortable as anywhere. There is no better holiday in our opinion than a fortnight spent between Belfast and the Giant's Causeway, with a day or two spared for the Mourne Mountains. These guide-books cover the exact area, and are indispensable each in itself and as supplying the gaps in the other. The first on our list is compiled by the well-known President of the Dublin Naturalists' Field Club, and that alone should secure it a large sale. It deals in an equally able manner with the History, Archaeology, Sports, Artistic merits, Natural History, and Geology, and these in no mere perfunctory way. It has several maps, one of which, by Professor M. F. Fitzgerald, is of great value, as it gives for the first time the

contours of Newcastle district of the Mourne Mountains. There is scarcely need to mention the photographic reproductions of Mr Welch, every reader of *Natural Science* must be familiar with them; but it may be as well to note that the original photographs naturally far surpass these reproductions. For these alone it will repay every geologist to invest his shilling.

The other book is of quite similar class to the first, which, as we said above, it complements. Here the Geology is dealt with by Professor Cole, the Botany by Mr Praeger, Fishing by J. S. Hamill, Antiquities by W. Gray, and Sketching localities by Miss Sydney Thompson. An excellent and liberal selection of Mr Welch's views are also drawn upon, and many other pictures of interest. We note the exterior and interior of the homestead of Francis M'Kinley, whose descendant is now President of the United States. Like the above book, this is of especial interest to geologists by reason of its pictures, and we strongly recommend it to any one who has seen or is interested in the district of Larne to the Giant's Causeway.

Vitalism

During the recent academical year Prof. Léo Errera, of the Université Libre of Brussels, lectured on the question—"Is there such a Thing as a Vital Force?" He has been good enough to send us a syllabus of his lectures, which contains a very useful bibliography of the subject. He comes to the conclusion that there has not yet been demonstrated in living beings any source of energy independent of external energies, although the resultant of these various energies, as exhibited in the structure of an organised being, may conveniently be spoken of as 'vital.' To abstract what is itself an abstract is hardly possible for us; but those who are interested in the subject might do worse than write to the Libraire Lamertin, Rue Marché-au-Bois, Bruxelles, for a copy, which costs 75 centimes.

The zoological results of Dr Arthur Willey's travels in New Britain, New Guinea, the Loyalty Islands, and other islands of the South Pacific, during 1895-97, are to be published by the Cambridge University Press in a series of monographs. The writers, besides Dr Willey himself, include Dr Paul Mayer, Mr R. I. Pocock, Dr D. Sharp, Prof. S. J. Hickson, Mr A. E. Shipley, and Mr Jeffrey Bell. The work is expected to be completed in five or six parts, of which two will be issued during the autumn.

We have received from Messrs Blackie & Son a small manual, entitled "Elementary Chemistry, Practical and Theoretical, First Year's Course," by T. A. Cheetham. Since this is outside our scope we can only say that Mr Cheetham appears to have combined the practical and theoretical divisions of his subject in an intimate and successful manner. The descriptions of the experiments are clear and should enable them to be carried out easily by the student.

Scraps from Serials

To the paper of Mr Jukes-Browne, which appears in its English form in the present number, a reply by Mr G. Dollfus, entitled "Rôle de la

stratigraphie dans la classification Géologique," was published in the August number of *La Feuille des jeunes naturalistes*. Evidently the discussion has cleared the ground, and though the compromise suggested by Mr Dollfus is not likely to be accepted by Mr Jukes-Browne, there are signs that an agreement may be arrived at eventually.

In the *Westminster Review* for August, Mr J. F. Hewitt has an article on "The Smithsonian Institution: Its history and its later Ethnological publications."

No. 4. of vol. iii. of the *Records of the Australian Museum* was published on June 13, and contained a description of new or little known Palaeozoic Gastropoda from Victoria, Tasmania, and N.S. Wales, referred to the genera *Goniostropha*, *Mourlonia*, *Helicotoma*, *Trochonema*, *Holopea*, and a new genus *Gyrodoma* allied to *Murchisonia*, by R. Etheridge, jun., who also founds a new species *H. australis*, for specimens of the Silurian chain-coral, *Halysites*, from N.S. Wales. In the same number W. J. Rainbow describes the larva of the geometrical moth *Pseudoterpna percomptaria*, and a new species of Araneid, *Poltys multituberculatus*. C. Hedley describes and figures *Lima alata*, a new bivalve from Santa Cruz, S. Pacific. A. J. North furnishes a series of ornithological notes.

Part 2. of vol. ii. of *Annotationes Zoologicae Japonenses* is to hand, and contains a paper by C. Sasaki on the wild and domestic silkworms of Japan, in which he comes to the conclusion that the latter are derived from *Theophila mandarina*. Prof. Ijima contributes a preliminary synopsis of the genera and and species of the sponge family Rossellidae. Yet another preliminary notice we regret to see is that of new Japanese Echinoids by S. Yoshiwara; in this ten species are somewhat briefly described without any illustrations.

Volume iii. of the *Journal of the Essex Technical Laboratories* fully carries out the promise of earlier volumes, and Mr Houston and his assistants are to be congratulated on the success that has attended their efforts in the teaching of science as applied to agriculture and dairy-farming. The lectures on dairy bacteriology are particularly valuable.

Further Literature Received

Outlines of Vertebrate Palaeontology, A. S. Woodward: Cambridge, Nat. Sci. Manual. Natural Hygiene, Lahmann; Radiation, Hyndman: Sonnenschein, London. Plant Life, Barnes: Holt, New York. Classification of Vertebrata, Gadow: Black, London.

The Periodical Cicada, Marlatt: *Bull. U.S. Dept. Agriculture.* Minnesota Botanical Studies, ser. ii., part 1. Report Manchester Micro. Soc., 1897. Clays in New Jersey (no reference given), and Soundings from the Pacific, Edwards: *Amer. Micro. Journ.* Importation of San José Scale from Japan, Webster: *Canadian Entomologist.* Transactions Norfolk Nats. Soc., vol. vi., part 4. Report S. African Mus., 1897. Flat-fishes of Cape Colony, Boulenger: Dept. Agriculture, C. of Good Hope. L'origine des individus, Herrera: *Mem. Soc. Antonio Alzate.* The Temperance Question, Reid: *Medical Mag.* Report Manchester Mus., 1897-98. Seams of Lancashire Coal measures, Bolton: *Manchester Mus. Handbook.* Rev. Mensuelle de Bibliogr. Scient., Baillière.

Amer. Journ. Sci., Aug.; Amer. Micro. Journ., June, July; Amer. Nat., June, July; L'Anthropologie, May-June, vol. ix., No. 3; Avicula, May-June, July-Aug.; Boll. de Naturalista, xviii., No. 7; Botan. Gazette, July; Feuille des jeunes Nat., Aug.; Irish. Nat., Aug.; Knowledge, July, Aug.; Literary Digest, July 9, 16, 22, 30, Aug. 6; Naturalist, Aug; Nature, July 21, 28, Aug. 4, 11; Nature Notes, Aug.; New Age, June; Photogram, Aug.; Plant World, July; Review of Reviews, July; Revue Scient., July 23, 30, Aug. 6, 13; Riv. Ital. Sci. Nat., July and Aug.; Rev. polit. e litteraria, iv., fasc. 1; Science, July 8, 15, 22, 29, Aug. 5; Scientific Amer., July 9, 16, 23, 30, Aug. 6; Riv. Psichologia, June, fasc. 4 and 5, July, fasc. 6; Scot. Med. and Surg. Journ., Aug.; Scot. Geogr. Mag., Aug.; Victorian Nat., June, July; Westminster Rev., Aug.

NEWS

Professor E. Ray Lankester has been appointed Director of the Natural History Branch of the British Museum. We have on a previous occasion expressed our admiration of Prof. Lankester's biological work. Further than this he has shown, by the excellent museum installations carried out under his guidance at Oxford, how well he is fitted to continue the exposition of the national collections on the lines laid down by Owen and Flower. All who have ever had the pleasure of working under him admit with gratitude the inspiration and guidance they have received, and recognise that his powers are remarkably adapted for the direction of a great scientific establishment. We fail to see how a more fitting person could have been found.

Dr D. Morris, of Kew, has been appointed head of a newly founded government department to direct practical applications of botany in the West Indies. We fear that the learned gentleman will not be welcomed with open arms by the many botanists in those parts, which already have an excellent Botanical Garden and staff in Jamaica.

Other recent appointments are those of Dr Charles Hunter Stewart to the professorship of Public Health and Sanitary Science at Edinburgh University, and Dr Heinrich Ries, of Columbia University, to be instructor in Economic Geology, a newly created post at Cornell University.

After serving the Museum of Comparative Zoology in various capacities for thirty-five years, Mr Alexander Agassiz has resigned his position as Director and Curator. The policy of the establishment will hereafter be guided by a Committee of the Museum Faculty of Harvard, consisting of Dr H. P. Walcott and Professor George L. Goodale. Dr W. McM. Woodworth has been appointed assistant in charge of the Museum, to date from August 1, 1898.

Professor James Hall, State-Geologist of New York, died on August 7th, having nearly completed his eighty-seventh year. We hope to devote a special article to this famous American geologist in our next number.

The new galleries of comparative anatomy, anthropology, and palaeontology, at the Museum of Natural History, Paris, were opened on July 21.

The U.S. National Museum has recently acquired the Lacois collection of fossil insects. It is said to contain over 6000 specimens.

During two years of active work in amassing an herbarium in connection with the Botanical Department of the Field Columbian Museum, Chicago, over 50,000 mounted and classified sheets have been accumulated; these are distributed geographically about as follows:—North America, 16,000; Mexican Boundary, 1575; Mexico, 6125; Central America, 1575; West Indies, 1050; South America, 1500; Europe, 10,500; Asia, 4500; Africa, 3850; Japan, 1050; Oceania, 1200; Australasia, 2250.

We regret to learn that the young male giraffe recently bought by the Zoological Society died on the night of Monday, August 8, from indigestion.

The Lincolnshire Naturalists' Union, joined by members of the Lincolnshire Science Society, and the Grantham, Grimsby, and Louth Naturalists' Societies, met at Grantham on June 7, and had a very successful excursion to Saltersford, Stoke Park, Woolsthorpe Manor House, and Little Ponton. A report by Rev. E. A. Woodruffe-Peacock, with lists of specimens collected, appeared in *The Naturalist* for August.

On August 1 the Yorkshire Naturalists' Union made an excursion to Spurn, and succeeded in collecting many specimens of interest.

The Geologists' Association of London held their Long Excursion in the Birmingham district from July 28 to August 3, under the Directorship of Professors Lapworth and Watts, Dr Stacey Wilson, and Messrs Jerome Harrison and Wickham King. Messrs Sollas, Blake, Sherborn, and Miss Wood of Birmingham, were among the fifty or sixty persons present. Mr Frederick Meeson acted efficiently as Excursion Secretary. The main attraction of the Excursion was the comparison of the Archaean and Cambrian Rocks of the district with those seen on a previous occasion in the Shrewsbury area under the same directors. The clear and patient exposition of the "Old Boy," as Professor Lapworth calls the Archaean rock, was warmly acknowledged by the visitors, many of whom had followed for a second time this eminent leader in British geology. The basic dyke in Abel's Quarry, near Nuneaton, penetrating the Archaean, but cut off by the overlying Cambrian Quartzite, was an object of much interest, while the Hyolithes beds of Cambrian age yielded sparingly *Kutorgina*, *Hyolithes*, and other fossils. The remarkable bending of the edges of the Menevian beds underlying the Carboniferous conglomerate was examined in detail, and the theory of the movement of soil-cap was held to be sufficient to account for it without calling in any more violent means. The geologists were shown, by Professors Lapworth and Watts, the imaginary restoration of the old Triassic sea, with its islands of Charnwood, Nuneaton, Lickey, Shrewsbury, &c. The head of a trilobite was found for the first time in the Lower Stockingford Shales, thus helping forward the elucidation of the life of the period. The last day an excursion was made to the Dudley and Wren's Nest Silurian, and owing to the excellent arrangements made by Mr Claughton the workings were explored in boats in a most complete manner. Heavy bags were made, chiefly of rock-specimens, and a rumour was current a few days later that the Oxford express had broken an axle.

The German Emperor, whose sympathy with all forms of literature, art, and science is notorious, must have had excellent reasons for prohibiting the intended meeting at Posen of the Polish Association for the Promotion of Medical and Natural Science, and for threatening with banishment any Prussian subject who should take part in the proceedings. That he should wish "to envenom the relations betwen the Polish and the German nationality," no sane man can believe ; yet this is what is openly stated by sixty protestant professors in a circular addressed to leading members of the medical profession in Great Britain.

At the Congress of Chemists in Vienna, Dr Leo Lilienfeld, a former pupil of Du Bois Reymond, and now at Vienna, demonstrated a simple synthesis of albumen, or rather pepton, said to be similar in composition and reactions to the natural product as formed by the digestion of albuminous substances. The ingredients are said to be phenol, glycocol, amydo-acetic acid, monochlorine acetic acid, and phosphoric oxychloride. Whether the nutritive qualities of the compound are the same as those of natural pepton has yet to be proved. There is no immediate prospect of its replacing the roast beef of old England.

From the *Bolletino del Naturalista* we learn that a committee has been formed at Turin in order to establish there a freshwater aquarium for the advancement of pisciculture in Italy and specially in Piedmont.

On August 1 the Public Library at Norwich was destroyed by fire. It had been founded over a century, and contained more than 60,000 volumes. The department of local archaeology was specially valuable, and many of the books so unhappily lost can never be replaced.

The expedition of Mr C. E. Borchgrewink to the Antarctic is to sail early in October on board the 'Southern Cross,' built by Mr Colin Archer, architect of the 'Fram.' The scientific staff includes Sub-Lieutenant William Colbeck, R.N.R., and Mr Louis Bernacchi of the Melbourne Observatory, as magnetic officers; Dr Herlof Klövstad of Christiania Observatory, as medical officer; Messrs Nicolai Hansen and Hugh Evans, as zoologists and collectors.

The Egyptian Geological Survey proposes to attack the Peninsula of Sinai during the coming winter. From Dr W. F. Hume, who, with Mr Skill as topographer, will survey regions as yet little explored, we look for some interesting results.

The Congo Independent State intends to make a thorough scientific survey of Tanganyika. Twenty observation and experiment stations have already been built, and collections will be made of the flora, fauna, and geological specimens. The results have to be published at Brussels in a new periodical, the *Scientific Annals*, which will appear every six weeks.

An expedition is being fitted out in Amsterdam for the zoological, botanical, and oceanographic exploration of the waters of the East Indian Archipelago. The leader will be Dr Max Weber, professor of zoology at Amsterdam University. He will be accompanied by Mrs Weber, who will have charge of the botanical section of the researches, and by Dr J. Versluys and Mr H. F. Nierstrasz, who will assist in zoology.

The German deep-sea expedition, on the s.s. 'Valdivia,' of the Hamburg-American Line, Captain Krech, left Cuxhaven at 8 P.M. on August 1st, and crossed to Granton. Some successful trials of the apparatus were made on the way. The scientific staff consists of Prof. Chun of Leipzig, director; Prof. Schimper of Bonn as botanist, Drs Apstein and Vanhoffen of Kiel, and Dr Braem of Breslau, as zoologists; Dr G. Schott of Hamburg as oceanographer; Dr P. Schmidt of Leipzig as chemist. Navigating officer Sachae, Dr Bachmann of Breslau as bacteriologist and medical officer. Non-official members are the zoologists, Dr Brauer of Marburg a/L., and Dr zur Strassen of Leipzig; and Mr F. Winter of Frankfort, as draughtsman and photographer. The laboratories and cabins are spacious and admirably fitted up, and the ship is supplied with a fine scientific library. On the evening of August 4 the 'Valdivia' again sailed for the Faroe Channel; she will pass round the north of Scotland, and then go down to Cape Town, where she is due towards the end of November.

CORRESPONDENCE

PROGRESS AND PROVENDER

Mr Bernard modestly considers that the chief value of his entertaining paper "A new reading in the Annulate Ancestry of the Vertebrata" (*Nat. Sci.*, xiii., pp. 17-30, July 1898) lies in its exemplification (whether rightly or wrongly) of a factor in evolution hitherto not sufficiently emphasised, namely, "that the profoundest morphological transformations leading to the rise of new groups of animals can be traced to the adoption of new methods of feeding." Self-evident though the proposition seems, "yet I am not aware," he continues, "that it has ever been applied systematically except in the two cases in which I have myself endeavoured to apply it." From this it appears that he is ignorant even of the title of Mr A. T. Masterman's suggestive article "On some points in the general morphology of the Metazoa considered in connection with the physiological processes of alimentation and excretion" (*Zool. Anzeiger*, xix., pp. 190-198, 206-221, and 225-229—1896). On p. 228 of that paper, Mr Masterman says, "Reasons have been given for regarding the modifications of the alimentary processes to be the direct originators of other sets of organs, the instances of skeletal and pigmentary organs being taken as typical. If in phylogeny the various organs arise from and are intimately connected with, the alimentary processes, then in ontogeny the same will result. The first signs of differentiation will appear in connection with the sustentative function, and mechanical ingestive processes will lead the way."

That modifications of the alimentary processes have been the chief guides in the evolution of the classes of Echinoderma is a view that I never felt to be in need of emphasis. It has however been emphasised by Dr Otto Jaekel, who says, "The morphogeny of the Pelmatozoa depends essentially on two factors, on the one hand the development of the nutrient ciliated grooves of the ambulacra, which soon results in the formation of free arms, on the other hand those passive transformations which bring about a correlation of those structures with the rest of the body" (*Sitzber. Ges. naturf. Freunde Berlin*, 1894, p. 103).

Mr Bernard will doubtless be glad to find that a view which he holds so strongly and expresses so ably, is not likely to perish for lack of other support.

F. A. Bather.

"THE STUDY OF VARIATIONS."

While not wishing to unnecessarily prolong this subject, I should be glad if you would allow me to clear up some misconceptions in reference to the position I endeavoured to maintain in your magazine for April and June.

My position was based mainly on the immense difficulty experienced in determining the value of *small variations*, and the fact, equally patent, that when this had been accomplished there remained even more disputed questions in reference to the causes which had led to their production.

Instead of offering any theory of heredity, I merely put forward some suggestions which I thought might explain the causes of this endless dispute: for this reason I endeavoured, as far as possible, to approach the subject from a neutral position. As a matter of personal belief I think Mr Henslow and others have succeeded in demonstrating that variations are more definite in nature than Darwin believed. I did not need to state this conviction, because I *provisionally accepted* the facts adduced by Mr Henslow himself, which therefore made it quite unnecessary to bring forward any other evidence.

While admitting that his explanation was, from his point of view, justifiable, it yet appeared to me that the *same facts* were capable of explanation on another theory, which was itself merely the *necessary* corollary of Natural Selection. Thus merely by continually eliminating the less fit, and therefore leaving the more fit to survive and reproduce, the average range of variability must increasingly tend towards perfect adaptability, in exact proportion to the length of time and constancy of conditions operating on the organism. On this hypothesis those forms of life which are subjected to continuous conditions should have variations which are more definite in character than other forms which have a more varied and less constant environment, hence plant life generally as compared with animal should exhibit greater evidence of definiteness in its variability,

and because conditions are usually more powerful, and necessarily operate for shorter periods of time, when under man's direction than in nature, so variations should be correspondingly less definite in the domesticated forms of life.

This position also destroys the force of the two chief arguments used against Natural Selection by demonstrating *that it is itself able to induce those favourable variations, which it subsequently selects from.* Firstly, it is urged that Natural Selection being unable to produce definite variations must be dependant on some other factor until the variations are sufficiently far advanced to be of selective value, and it is therefore incompetent to solve the most important question in the formation of species ; secondly, the number of coincidences, which are necessary to perpetuate any given favourable variation on the assumption of *indefinite variability*, are so great that Natural Selection must be regarded, at best, only as a subordinate factor. By showing that, if natural selection acts at all, it must tend to produce definite variations, these two objections are largely overcome, and the *facts* adduced by the Neo-Lamarckian school easily accounted for. Hence with two competing theories to explain species formation, it becomes necessary to make a further appeal to facts to determine the value of each.

I feel so convinced of the importance of this aspect of the subject that I should be sorry if any want of clearness on my part at all obscured the point at issue.

J. Lionel Tayler.

The Grotto, Hampton-on-Thames.

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 29 and 30 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

To the Trade.—Natural Science is published on the 25th of each month ; all advertisements should be in the Publishers' hands not later than the 20th.

To our Subscribers and Others.—There are now published Twelve Volumes of Natural Science. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being out of print, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at One Shilling each.

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OCT 24 1898

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 80—Vol. XIII—OCTOBER 1898

NOTES AND COMMENTS

Wanted, an Editor!

It is with the deepest regret that we find ourselves compelled to announce a step which may, we fear, result in the cessation of this Review at the close of the present year. For a time that, as the history of journals goes, is short, but that, when taken from the life of individuals in its most active period, appears long indeed, we have endeavoured to maintain the fortunes of *Natural Science*. The labour that this has entailed has undoubtedly been one of love; but, as years advance, responsibilities increase, and the time at the disposal of those who conduct this Review becomes less. We have therefore decided to discontinue the editing of *Natural Science* after the next December number.

It may be imagined that we do not take this step without anxious deliberation. We believe ourselves, and we are told by others, that *Natural Science* has filled a place in scientific journalism occupied by no other periodical, at least in this country. We have endeavoured to be independent, an aim that it is often difficult to accomplish. We have sought to praise the good because it was good, and to censure the bad because it was bad. And if censure has sometimes seemed to overbalance praise, let the truism be remembered that there is far more bad than good in the world. We have endeavoured to be impartial and to bow to no authority save justice and reason; but we have also tried to recognise that our ideas of justice and reason might not always be those of other people. Hence we have allowed a free field to the champions of views unorthodox as well as orthodox. The reproach of omniscience and infallibility has, it is true, been laid to our charge. But these sins of a private individual are the virtues of an editor. They may be accounted for by the fact that we have had freely placed at our disposal the pens of many, if not most, of the eminent biologists and geologists of the day, without distinction of nationality. To all our contributors, those whose names have been published, and those who have helped in the less grateful task of furnishing unsigned comments, we tender our heartfelt thanks.

We have said this much in praise of *Natural Science*, because we believe that a journal of this nature should not be allowed to drop altogether. We are prepared to hand it over, with all stock, appurtenances, and goodwill, to any scientific man who is prepared

to relieve us of all responsibility and to continue it as an independent journal. We had rather see it continue in other hands than drop out of existence altogether, and we believe that our opinion will be shared by most of our readers. Now is the time for expressions of admiration and sympathy to be translated into practical aid.

The Biological Exhibition at Bristol

Among the features of the British Association meeting that held out promise of novelty and interest was the widely-advertised Biological Exhibition arranged at the Zoological Gardens, Clifton. At 3 p.m. on September 8th, the entrance to the grounds was crowded by those who came to hear and see Sir John Lubbock open the exhibition. These numbers can hardly have been expected by those responsible for the arrangements, and it resulted that Sir John was seen by few and heard by fewer. The exhibition itself was attractive in many ways, and, considering the intense heat of these few days, remarkable as a *tour de force*; but to the biologist it was rather disappointing. The greater part of it was a flower-show, to which many leading firms of florists contributed. In this section the exhibit of most scientific interest was that of Dr E. J. Lowe, who showed beautiful examples of rare ferns, with crossed varieties of double, triple, and quadruple parentage. The zoological section contained a somewhat miscellaneous lot of exhibits, and it was clear that the committee had been prevented by the usual considerations from exercising that stern selection which alone could have maintained the desired standard. We, who are not thus hampered, need only mention the following:—Dr J. A. Norton of Bristol showed clutches containing cuckoos' eggs, in illustration of the various foster-parents, also a series of robins' nests with cuckoos' eggs, intended to elucidate the problem whether there be any variation in the egg according to the nest in which it is laid; Mr C. K. Rudge of Clifton, various nests of British birds, with an analysis of the materials from which they were formed. Mr G. C. Griffiths of Clifton had an interesting little case containing hybrids of Lepidoptera, examples of mimicry of plants by Lepidoptera (*e.g. Kallina inachis*, the Indian leaf-butterfly and its allies), and instances of protective and aggressive resemblances among Orthoptera. Mr H. A. Francis of Clifton showed specimens of local wasps and wild bees, with examples of protective mimicry of the same by other insects. Professor E. B. Poulton exhibited the proof quite recently obtained by Mr Guy A. K. Marshall, that *Precis octavia-natalensis* and *P. sesamus* are but seasonal variations of the same species: the parent specimen, here exhibited, of the form *P. octavia-natalensis* laid three eggs on February 27, 1898; on April 15, one of these produced a *P.*

sesamus, here exhibited; and on April 20, another egg produced *P. octavia-natalensis*, also shown. Mr F. G. Richmond of Braunton, Devon, had arranged some aquaria containing various species and hybrids of trout and salmon. The exhibit of the Marine Biological Association was of interest, not so much for the species shown as for the illustration it afforded of success, under the most difficult conditions, of keeping marine animals of various kinds alive by the constant circulation of the water. There were five shallow wooden tanks and about 400 gallons of sea-water was used. The water entered the tanks from a small upper reservoir by means of glass siphons, and after passing through them collected in two lower reservoirs, from which it was periodically pumped into the upper one again. The pump used was an ordinary semi-rotary yacht pump. Most of the fish, as well as many of the invertebrats, had been living in a healthy condition in the tanks for some two weeks previous to the opening of the exhibition, and there had been comparatively few deaths. Naturalists were also glad to have the opportunity of seeing in operation the ingenious plungers devised by Mr E. T. Browne and Dr E. J. Allen, and worked automatically by means of the fresh-water supply filling a can that was intermittently emptied by a siphon. Such a plunger keeps the sea-water in a bell-jar aquarium in constant movement, and serves to keep medusae alive and to rear the larvae of marine animals to the adult stage.

Bread out of Air

Sir William Crookes in his presidential address to the British Association traversed a wide field, practical, physical and psychical, in each department applying or suggesting the application of the most recent advances in modern physical research.

Starting with the question of late so much mooted, of the wheat-supply of these islands, intimately connected with the wider question as to the possible insufficiency of the wheat-supply of the whole world at no distant date, Sir William showed that whereas the consumption of wheat was increasing, the amount of land available for its production was strictly limited, and that already those limits were nearly reached. It is therefore necessary to increase the fertility of the land by means of nitrogenous manures. Unfortunately that special sanitary appliance which is the glory of our country, and which she has presented to all parts of the civilised world, has the disadvantage of wasting, in this country alone, "fixed nitrogen to the value of no less than £16,000,000 per annum." But even if we relinquish the system that Liebig stigmatised as "a sinful violation of the divine laws of nature," we shall not repair the mischief already done. Already the world's

deposits of guano are becoming exhausted, and our final resource, the nitrate-fields of Chile, cannot last more than fifty years, even if the demands upon them be not increased. The solution proposed by Sir William Crookes is the formation of nitrate by the combustion of the atmosphere. This can be effected, he says, by passing a strong induction current between terminals. By utilising natural sources of power, an electric current may be obtained that will enable nitrate of soda to be produced at less than £5 per ton, two-thirds the price of Chile nitrate.

The Mysteries of Matter

The second portion of the Presidential address was interesting chiefly for its speculations on the constitution of matter. Here Sir William adduced the various recent discoveries confirmatory of his own views, so long opposed, as to the existence of molecular streams of electrified radiant matter. The task of the future is to render available the energy contained in matter that to outward appearance is quiescent. The phosphorescence of uranium, and in a higher degree of the newly discovered polonium, is due to its power of extracting such energy, however that power be explicable. A new application of the principles that underly his theory of radiant matter has within the last few weeks enabled Sir William Crookes to add another to his remarkable successes in the fractionation and spectrographic study of the rare earths. He believes that he has demonstrated the existence of yet another element, which he terms Monium, because the lines of its spectrum stand alone, almost at the extreme end of the ultra-violet.

These far-reaching speculations as to the existence of energy, barely thinkable and yet capable of investigation, of measurement, and of utilisation, naturally led Sir William Crookes on to the most debateable and most confessedly speculative part of his address. To his previously published statements on the subject of psychical research, he adheres. However far we may accompany or lag behind Sir William in acceptance of the alleged phenomena of telepathy, this at least we must recognise in his words: the belief on the one hand that the enquiry has not yet reached the scientific stage of certainty; on the other hand, that any explanation will be an extension of theories of the constitution of the material universe already widely held and serving as the basis of actual experiment. Whether the suggestion, which we imagine to be implied, that telepathy is analogous to wireless telegraphy be accepted by physiologists matters little. We believe that it is right of Sir William Crookes to allude to these matters from the President's chair, since we think that for their investigation is demanded the co-operation of the keenest intellects in all branches of science. "To ignore the

subject would be"—not only for Sir William Crookes—"an act of cowardice."

Stereochemistry and Vitalism

In connection with the above remarks, it is interesting to note that the Presidents of the Sections for Mathematical and Physical Science and Chemistry—Professors Ayrton and Japp—dealt with subjects which might equally well have been considered in the Biological Sections, while the President of the Zoological Section applied to certain biological problems the methods of mathematics. Professor Ayrton described certain interesting experiments on the smells of substances. Professor Japp considered certain facts of stereochemistry in their relation to the fundamental problem of life. He pointed out that all inorganic compounds were symmetric, and that the forces producing them were either symmetric, or, if asymmetric, then asymmetric in two opposite senses. Compounds of one-sided asymmetry originate with the living world, and are only known to be produced by some selection in which living organisms must directly or indirectly take part. It is of course possible that some day the isolation of asymmetric compounds may be proved possible without the intervention of even the directing intelligence of the chemist in his laboratory. Professor Armstrong, who spoke after the address, evidently thought that this would be proved before the next meeting of the British Association in Bristol. We are as yet only on the threshold of the problem, and fresh methods of investigation or fresh conceptions may upset the prophecies of to-day. For the present, however, Professor Japp has done good service in setting before us one of the difficulties to be overcome before the vitalistic hypothesis can be rejected.

The Need of Numerical Investigation in Biology

Professor Weldon, choosing a subject which the ordinary naturalist is apt to consider abstruse and uninviting, succeeded in delivering an address that both for content and exposition was one of the successes of the meeting. Entitled, "Some objections to the theory of Natural Selection," it was in the main an attempt to expound to biologists the modern doctrine of chance, and to show that the variations which actually occur in the animal world are neither more nor less definite than those which result in the tossing of ha'pence or the casting of dice; further, that these 'chance variations' do afford scope for the action of Natural Selection in a way that admits of accurate measurement. The instance taken was the variation in frontal breadth of *Carcinus mænas* from a particular patch of beach in Plymouth between 1893 and 1898. It was shown that the frontal breadth was diminishing at a rapid rate in this particular race, and evidence was adduced to prove that this

was due to the selective action of an increase of silt in the water. Professor Weldon urged the necessity of extending as widely as possible this kind of numerical study. The difficulty of the theory of Natural Selection lies in the postulate that in any given case a small deviation from the mean character will be sufficiently useful or sufficiently harmful to affect the race. But determination of the deviation and of its effect on the death-rate is often possible. Whenever possible it is our duty to make it. "Numerical knowledge of this kind is the only ultimate test of the theory of Natural Selection, or of any other theory of any natural process whatever."

Morphology

Professor Bower, in his address to the Section of Botany, placed before his hearers the principles of modern morphology, and discussed the limits of their application. He advocated the establishment of classifications upon purely phylogenetic grounds. The attempt is beset with difficulties of all kinds, but it is the only goal of the taxonomist. Now this transference of our point of view from mere similarity of structure to questions of the origin of each structure brings into still greater relief the ever more complicated problems of homology. When we find that organs, structurally similar, have been independently developed in totally different races, how far can we consider them homologous? Ought we even to call them by the same names? The difficulties are manifest enough in every group of animals and plants; but often they are complicated by an alternation of generations, in which case the use of identical terms for organs that arise at absolutely different stages of life-history is apt to give rise to serious misconception.

"Taking the case of leaves for the purpose of illustration, we may contemplate the following possibilities:—(*a*) A possible origin of two homoplastic series of leaves in the same plant, and the same generation (*Phylloglossum*); (*b*) Two homoplastic series in the same plant, but in different generations (*Lycopodium cernuum*); (*c*) a possible distinct origin of homoplastic leaves in distinct phyla, but in the same generation (sporophyte of ferns, lycopods, equiseta); (*d*) a distinct origin of homoplastic leaves in distinct phyla, and distinct generations (*e.g.* leaves of Bryophyta and of Pteridophyta). Now *Homology* has been used in an extended sense as including many, or even all, of these categories. It seems plain to me that this collective use of the term homology carries no distinct evolutionary idea with it; it indicates little more than a vague similarity; the word will have to be either more strictly defined or dropped. The old categories of parts based upon the place and mode of their origin are apt to be split up if the system be checked by views as

to descent. Comparison, aided by experiment, supersedes all other methods, and the results which follow raise the question of terminology of parts which have arisen by parallel development. In parts which are of secondary importance, such as stipules, pinnae, the indusium, hairs, glands, the inconstancy of their occurrence points to independent origin by parallel development in a high degree; in parts of greater importance, such as leaves, a parallel development may also be recognised, though in a less high degree; in the case of sporangia, their acceptance as a category *sui generis* dispelled the old view of their various origin from vegetative parts; but we must remember that this does not by any means exclude a parallel development also in them, by enlargement and septation from some simpler spore-producing body, though this is not yet a matter of demonstration. There are two extreme courses open to those who wish to convey clearly to others such matters as these: the one is to use a separate term for each category of parts, which can be followed as maintaining its individual or essential identity throughout a recognised line of descent—in fact, to make a polynomic terminology of members run parallel with a polyphyletic development; the other course is to make it clear always in the use of terms applied to parts, that they do not contain any evolutionary meaning, and to use them only in a descriptive sense. Perhaps the former is the ideal method, and it may be a desirable thing, as polyphyletic origins of parts become more established, that the terminology should be brought to reflect at least the more important conclusions arrived at. For the present, the whole matter is still so tentative that it is well to be content with something which falls short of the ideal, and to maintain the usual terms, such as stem, leaf, root, hair, sporangium, &c., as simply descriptive of parts which correspond as regards general features of origin, position, and nature; but with no reference either, on the one hand, to conformity to any ideal plan, or, on the other, to any community by descent—in fact, we shall preserve the original pre-Darwinian sense of these words, which was purely descriptive, and avoid any attempt to read into them any accessory meaning."

Form and Function

"And it was full of bones; . . . and lo, they were very dry."—Ezekiel.

One of the most serious drawbacks to the study of Botany as it is put before us in recent text-books, especially English text-books, is the dry unlifelikeness of the part devoted to Morphology. And especially external morphology, the comparative study of the general form and development of the plant-members, which, completely divorced as it has been from the study of function, reserved for the chapter on physiology, has often degenerated into a succession of

pages filled with theory and bristling with technical terms. Dr Goebel, professor at Munich University, is to be congratulated on the attempt in his recently published *Organographie der Pflanzen* (part 1) (Fischer, Jena) to clothe and breathe the breath of life into those dry bones. Taking his text from Herbert Spencer when insisting on the interdependence of structure and function, and the impossibility of giving any true explanation of natural phenomena without keeping in view this co-operation, he puts before us a system of morphology based on physiology and biology. As in the case of most reformers he sometimes goes too far. Few botanists will follow him, when, for instance, he sets aside entirely homologies of stem and leaf-structures, preferring instead physiological analogies as a basis of terminology.

Notwithstanding such occasional examples of over-zealousness, Prof. Goebel's book is a most useful addition to botanical literature, and should be read and marked by all advanced students and teachers of botany. The subject matter falls under five sections, namely, general segmentation of the plant-body; symmetry relations; difference in formation of organs at different stages of development; young forms, malformations and their significance for organography; and influence of correlation and external stimuli upon form. We are especially glad to note the clear-headed treatment of the section on malformations which have been too much pressed into the service of morphology. Malformations, which by the way cannot be sharply distinguished from variations, follow certain laws, and are either inherited or caused by external factors. The study of monstrosities lends support to Sach's theory of 'Stoff und Form,' which insists that "differences in form of plant-organs are based on differences in material, and that alterations of form are due to alterations in the nutritive processes."

Rind Fungus and Sugar Cane

The Experimental Fields Station at Skerretts School, Antigua, has just issued a report by Messrs Francis Watts and F. R. Shepherd on the results obtained in the experimental cultivation of the Sugar Cane. These are a continuation of those which have been conducted since 1891, and comprise a study of a number of varieties of cane which have been established at the station for six years, an attempt to introduce additional varieties, and a record of results obtained on the applications of various manurial substances to the Bourbon cane in the hope of ascertaining the manurial requirements of the sugar cane under the conditions prevailing at Skerretts.

The chief result seems to be that the Rind fungus (*Trichosphaeria*) is a specific disease, because it cannot be attributed to a deficiency of lime in the soil as some writers have suggested. The disease

often occurs in places where the soil contains carbonates of alkaline earth equal to 40 or 50 per cent of carbonate of lime. As a general rule the authors seem to have found that the addition of Nitrogen either as Sulphate of Ammonia, Nitrate of Soda, or Dried Blood, or of Phosphates (mineral or basic rather than superphosphate) is beneficial, but at present it is very difficult to draw definite conclusions.

Agriculture in the United States

The excellent year-book of the U.S. Department of Agriculture (1897) of which we have recently received a copy shows a useful departure from the usual form. In addition to the miscellaneous papers, eighteen in number, there is a series setting forth the work of the several bureaus and divisions, under the general title "Work of the Department for the Farmer." No better means could be devised of proving to the American people, and peoples generally, the enormous advantages of such a department equipped with the best scientific experts that can be procured, and the great saving to the nation financially. Take for instance the first on the list, the 'Weather Bureau,' the work of which in relation to practical agriculture falls under three heads: (1) The forecast services for predicting storms, cold waves, and frosts. (2) The river and flood service for predicting floods. (3) The climate and crop service for recording and presenting the details of climate and the weekly and monthly conditions of crops. Besides the 150 paid meteorological stations there are no less than 3000 voluntary observers, the majority of whom, under the liberal policy of the Government, have been presented with standard instruments. By a wide distribution of weather forecasts and warnings, together with suggestions for minimising the injury arising from sudden meteorological changes or disturbances, crops, stock or property is saved each year many times exceeding in value the cost of the department.

The department of Botany refers to its work in introducing forage-plants suitable to the various parts of the country, in investigating fungous diseases of plants, in exposing the adulteration of seed, &c. The subject of weeds has been taken up with good results chiefly by preventing their introduction into uninfested parts of the country. Through a large number of correspondents the department is kept informed as to the distribution of the worst weeds, and maps showing at a glance their present range are constructed and kept on file. When information is received that one of these weeds has been found far beyond its known limits the local authorities are advised and the importance of promptly destroying it suggested, together with means by which the destruction can be accomplished. In this way the Russian thistle, which in 1893

damaged the wheat crop of the West to the extent of $3,000,000 to $5,000,000, has been kept out of California. Besides these two there are seventeen other papers by which the different bureaus and divisions equally justify their existence. One of the most useful items of the work of the department is the wide diffusion of its publications. Half a million copies of the present report are distributed, and the total number of publications issued during the past year was 424, aggregating over 6,500,000 copies. Yet the Secretary complains that he cannot nearly meet the growing demand, and asks for an increased appropriation.

Perhaps some day we shall have a Department of Agriculture on similar lines. In the meantime we would advise all who are interested in the application of science in this direction to buy or borrow a copy of the United States Year-book.

Recent Work on the Foraminifera

Since our note appeared in June last a great number of papers have come to hand. Foremost among these is one by R. M. Bagg, on "The Cretaceous Foraminifera of New Jersey" (*Bull. U.S. Geol. Survey*, No. 88). This paper, we believe, may be considered as the first serious paper on the group which has appeared in America, and is, moreover, well illustrated, and well edited. Bagg lists and describes about 110 forms, of which 6 are considered new. The work has been done from a zoological, not palaeontological, standpoint, and deserves warm praise. On the whole, we prefer Chapman's *Vitriwebbina* to the word *Vitrewebbina* used by the author. Chapman's contribution to recent literature deals with a new form from Torres Straits, which he calls *Haddonia*, one of the Lituolidae, related possibly to *Rupertia*. The paper appeared in *Journ. Linn. Soc. Zool.*, vol. xxvi. In the *Journal of the Royal Microscopical Society*, 1898, pp. 258-269, is a paper by that careful writer, F. W. Millett of Marazion, whose work, unfortunately, we so rarely see in print. It is, however, one of a series, which the Microscopical Society may well be congratulated upon having secured. Mr Millett deals with the Foraminifera of the Malay Archipelago, from material from thirty stations collected by Mr A. Durrand. At present only the *Miliolinae* have appeared, but there is promise of a series of especial value, and one which should be doubly welcome to those investigating the structures of Funafuti and Christmas Islands. The region from which this rich material comes was practically untouched by the "Challenger."

Dr Alfredo Silvestri publishes in the *Atti* of the *Accademie di Scienze Acireale*, vol. viii., a "Contribuzione allo studio dei Foraminiferi Adriatici," part 1 of which appeared in 1895. The work is the more valuable as it is the first thoroughly systematic descrip-

tion of the contents of the sands, so laboriously studied by Soldani, and rendered classic by the figures of Plancus, Gualtieri, Breyne and Ginanni. Silvestri also figures a new form of *Peneroplis pertusus* in *Memorie Pont. Accademie Nuovi Lincei*, vol. xiv.; but these one is tempted to regard more as worn specimens than novelties. They are however well figured on two plates, and form a useful contribution to the subject in any case. Carlo Fornasini is still busy, and gives us a beautiful plate of *Uvigerina bononiensis* in *Rivista Italiana di Palaeontologia*, anno iv., one specimen of which shows a double mouth. He also publishes another contribution to the Tertiary Foraminifera of Italy, dealing this time with the Pliocene of San Pietro, in Lama, near Lecco. His third paper, now before us, is "Indice de le Rotaliine fossili d'Italia," published by the Bologna Academy in its Memoires, and which is especially valuable in that Fornasini gives *facsimiles* of d'Orbigny's original drawings of his described species, which have never been published before.

Yet another paper is one by Jan Perner of the Prague Museum who describes and figures in the *Bulletin international, Academie des Sciences de Bohême*, 1898, some very interesting Lituoloids from the Tithonian of Stramberg. Five forms are described, of which three are considered new, and two are insufficiently known to be specifically determined. Mr Charles Schlumberger occupies our attention with two papers, one, in *La Feuille des Jeunes Naturalistes*, on *Involutina conica*, n. sp., an interesting form from the great oolite of Héronvillette near Caen. The specimens were obtained by heating the rock and then plunging it into cold water. His second paper is on a new genus, which he calls *Meandropsina* Mun.-Chalm., though we believe that this is the first appearance of the name in print. The genus resembles *Orbiculina*, is formed of three thicknesses of cells, the centre of which is composed of spiral chambers, starting from an initial sphere, and becoming concentric and circular: this is covered above and below by a layer of vermiform and meandriform chambers. The layer is imperforate, but the last chamber has numerous openings all round the disc. It is of Cretaceous age. Friedrich Dreyer contributes a magnificent monograph on *Peneroplis*, of 119 pages and four double quarto and one single quarto plates. This is a separately published work issued by Engelmann of Leipzig at 12 marks, and the numerous figures show plainly the immense variation among the foraminifera.

The Periodical Cicada

The latest entomological Bulletin (No. 14, n.s.) of the U.S. Department of Agriculture comprises an exhaustive account by Mr C. L. Marlatt of *Cicada septendecim*, the American Periodical Cicad. The bibliography of this famous insect goes back to the year 1633, when

the sudden appearance of a brood was believed to be the cause of a "kinde of pestilent feaver." Records of the insect, during nearly two centuries, over the eastern and central States, have shown that there are two races, a northern with a seventeen-year, and a southern with a thirteen-year life-cycle. A number of broods of each race have been registered, and their distribution being known, the future occurrences of the insect can be accurately forecast for the various districts. Careful observations of the larval and nymph stages have been made, and the changes undergone by the insect during its long underground life have been traced. When development is complete the nymphs of a brood leave the ground almost simultaneously, and an alarming swarm of cicads is the result. The perfect insects live but a few weeks, and are believed to take no food. The female makes cuttings in tree-twigs wherein she deposits her eggs; the newly hatched larvae fall to the ground and burrow immediately. The injury caused by the cicads is almost confined to their egg-laying incisions; though the larvae and nymphs suck sap from the roots of plants, their slow rate of growth and feeding prevents them from doing much damage. The life-cycle of this cicad is longer than that of any known insect, but Mr Marlatt makes the probable suggestion that other larger species of the family might be found to have even longer larval stages, could the course of their generations be accurately followed.

The Larva of Pelophila

In part 2 of the *Transactions of the Entomological Society* for the present year (pp. 133-140), Messrs W. F. Johnson and G. H. Carpenter make a contribution to the neglected subject of the life-history of the Coleoptera, by describing with figures the grub of the ground-beetle *Pelophila borealis*, which they have discovered in Ireland. The larva agrees with those of the beetle's nearest relations —*Nebria* and *Leistus*—in possessing a pair of long, mobile cerci at the hinder end of the abdomen, apparently a primitive character. The head of the *Pelophila* grub, however, is broad and quadrate, and the legs short, contrasting with the rounded head with constricted neck and long legs of *Nebria* and *Leistus*, and in these respects recalling the structure of more generalised carabid larvae.

Flat-Fish of South Africa

Mr J. D. F. Gilchrist, who was recently appointed Marine Biologist to the Government of Cape Colony, is publishing in separate papers descriptions by various specialists of the material which he collects. These are entitled, "Marine Investigations in South Africa. Department of Agriculture, Cape of Good Hope." We have received a copy of a short paper on the Flat Fishes by Mr G. A. Boulenger.

It contains the description of a species new to science, and descriptions of the five other species previously known from the coast of South Africa. The new species is an *Arnoglossus*, and receives the name *A. capensis*. In these descriptions, as in many others published by the systematists of the British Museum of Natural History, generic characters are unnecessarily repeated, and no attempt is made to point out the characters which distinguish the species from its nearest allies. The new *Arnoglossus* is described from a single specimen 16 cm. long. The sex of the specimen is not stated, nor is any mention made of the depth at which it occurred. Considering the interest that has been exerted by the sexual dimorphism of the British *Arnoglossus*, some reference to the subject might have been expected in the definition of a new species of the genus. In the case of the other species, beyond the statement of the specific characters, no details concerning the specimens are given. It is to be hoped that other specialists who undertake the examination of Dr Gilchrist's collections will describe the specimens, and not merely identify them, and will give specific definitions.

A New Ichthyosaurus

A fine skeleton of Ichthyosaurus has recently been uncovered in a quarry in the Lower Lias at Stockton, a village near Rugby. The owner, Mr Lakin, communicated with the authorities of the British Museum (Natural History), to which institution he has presented the specimen, and arrangements were made for carefully extracting it without disturbing the relative position of the bones. The animal lies in a clayey band, which, is unfortunately, unfavourable for the preservation of any traces of the outlines of the soft parts such as occur in the specimens described by Fraas. The only parts of the skeleton wanting are portions of the pelvic and pectoral girdles and some small bones of the paddles. The total length is about 18 ft. The quarry is said to have been visited by thousands of people, most of whom, no doubt, would not have taken the smallest notice of the skeleton had it happened to be in a glass case. The attention of some of these enthusiasts might perhaps be profitably directed to the County Museum where there are many fine fossils.

A New Dinosaur

Another still more important find of reptilian remains has recently been made by Mr A. N. Leeds, whose collections of vertebrate remains from the Oxford clay are so well-known. In this case, a large part of the skeleton of a gigantic Dinosaur has been obtained, including a series of twenty-six caudal vertebrae, sixteen feet long, a hind limb, the femur of which is four feet six inches long, parts of the fore limb and of the pectoral and pelvic girdles. It is

to be hoped that further excavations will lead to the discovery of other bones, and particularly of the skull. The parts at present known indicate the existence of a dinosaurian reptile which seems to be closely related to the American genus *Diplodocus* and to the well-known *Cetiosaurus* and *Ornithopsis* of this country. In fact it is possible that it will be found that all these genera are almost identical.

The Exhibition of Extinct Vertebrates

From the Annual Report of the American Museum for 1897 we learn a good deal as to the methods employed for familiarising the public with the skeletons and external forms of the great extinct vertebrata. In this Report attention is especially called by means of photographs to a skeleton of an Upper Miocene Rhinoceros, in which the perfection of the beautiful methods employed by Mr Adam Hermann are well seen. The entire skeleton is supported by steel rods which pass through the centre of the bones, only the two main supports being visible. This gives a very striking effect, and seems a desirable method, provided the bones themselves are duplicates and not types. We are of the opinion, however, that duplicates and only duplicates should thus be treated, and consider that described or figured specimens, or unique things, should never be sacrificed, for when once a bone is pierced and mounted it is inaccessible to the student, to whom it is of far more importance than the general public, who are quite satisfied with a plaster cast to look at. The plasterotheria of our museums are admirable as teaching objects, but the anatomist wants to handle and examine the real bone, and such should never be maltreated in any way. The mount of *Phenacodus* in the American Museum is an admirable example of what we mean, for there every bone can be removed for purposes of study. Among the plaster reproductions of external forms of animals this Report illustrates *Agathaumus*, *Hadrosaurus*, *Naosaurus*, and a highly amusing representation of a combat for the diamond belt between J.L.—we beg pardon—*Megalosaurus* (*Laelaps*, *Dryptosaurus*) *aguilunguis* and another of its kind, which is very real, and would, could it possibly be seen at the Aquarium, draw immense houses. The attitude of defence of the recumbent dinosaur is very striking, and the terrific lunge possible of the hind legs might fairly convert the attacking creature into a constellation.

Cretaceous Rocks in West Greenland

David White and Charles Schuchert accompanied the Peary Arctic Expedition of 1897 to the Nugsuak peninsula, West Greenland. They have now published a paper describing the Cretaceous series of that locality (*Bull. Geol. Soc. Amer.*, ix., pp. 343-368, pls. xxiv.-

xxvi., June 21, 1898), and the following are their chief conclusions. The Cretaceous and Tertiary rocks lie unconformably on a hilly basement of old crystalline rocks and early Cretaceous basalts, and reach at Atanikerdluk 3040 feet above the sea. Along the north side of the peninsula the Lower Cretaceous beds have an easterly dip, although the higher beds appear towards the west, probably in consequence of faults. The sediments appear to have been derived from the east, in which direction are few marine but some freshwater fossils. The deposition of sediment seems to have been continuous in some portion of this region throughout Cretaceous and early Tertiary times, although minor movements and erosion may have affected the beds before they were covered by the Tertiary basalt cap. The entire thickness of the sedimentary rocks is over 3500 feet.

These beds were divided by Heer into four series, on the basis of their vegetable contents. Of the lowest of these, the Kome series, developed on the north coast of the peninsula, a thickness of probably not over 700 feet is exposed above tide. The discovery of additional dicotyledons in the Kome series, from which hitherto only *Populus primaeva* was known, and which was regarded as Urgonian in age by Heer, casts serious doubt on the reference of those beds to so low a stage in the Lower Cretaceous. The flora as a whole is, however, to be compared with that of the Virginian Potomac formation, with some, perhaps the upper, portion of which the Kome series is probably synchronous.

The Atane series, hitherto not positively known on the north shore of Nugsuak peninsula, is clearly present at Ujarartorsuak with characteristic Atane plants. Farther west, at Kook Angnertunek and Niakornat, the dark homogeneous shale series probably represents both Atane and Patoot members of the Upper Cretaceous, since of the marine organisms found here some are identical with those occurring at Ata and Patoot, the typical localities for the two divisions of the Upper Cretaceous. The marine invertebrates from the Atane series, which Heer correlated by means of fossil plants with the Cenomanian of Europe, strongly indicate that the series is to be correlated with Fort Pierre and Fox Hills or Montana formation of the western United States. By means of its fossil plants the Atane series is so closely related to the Vineyard series of Martha's Vineyard, the Amboy clays of the Raritan region of New Jersey, or the uppermost Potomac of Alabama, as to furnish strong reason for the belief that the middle one of Heer's groups is the Greenland contemporary of the Amboy clays. The Patoot series, which appears lithologically and stratigraphically to be inseparable from the Atane series, contains at the same time many plants common in the upper part of the Amboy clays, with others allied

more closely to the higher Cretaceous floras, such as that of the Laramie. The Patoot series may perhaps be safely interpreted as constituting a palaeontological as well as sedimentary transition from the Atane series to the Tertiary. The thickness of the Atane and Patoot series (Senonian) is not less than 1300 feet and may considerably exceed this.

Tertiary and later Geology of W. Greenland

Many plants from Atanikerdluk were referred by Heer to the Miocene, but are now generally admitted to be Oligocene, and Messrs White and Schuchert suggest that they may even be of Eocene age. They further remark that the distinction between the floras of Heer's three Cretaceous series rests largely on minutiae of systematic description that cannot be upheld, a view that will doubtless be shared by most palaeobotanists.

There is some evidence for Tertiary erosion west of Niakornat. After this the entire region was covered by a great number of superimposed, approximately horizontal, non-columnar basalt beds of varying thickness and of great extent. Frequently 3000 feet of this basalt cap remains, while at Kilertinguak (6250 feet above tide) over 4000 feet is preserved. In certain regions numerous dikes intersect at varying angles the Cretaceous, Tertiary, and even the lower portion of the basalt cap, and are frequently found both forking and intersecting. Intruded basalts are not rare, especially in the Tertiary. The peridotite intrusive beds, about 350 feet thick, behind Kaersut, are probably of Tertiary age, as are also the other high intercalated basalts. At the time of the great elevation of the region, probably in the late Tertiary, the basalt cap, which, judged by the development on Unbekanntes Island, may have exceeded 7000 feet in thickness, most probably extended northwards in an unbroken sheet from the south of Disko Island to beyond the Svartenhuk peninsula, a distance of 250 miles.

The dissection of this great basalt sheet, the development of the Vaigat, the Umanak fiordal system, the isolation of Disko—in fact, approximately the present land topography of this coast—were accomplished at a much greater elevation during Pleistocene time. Evidence of post-Pleistocene subsidence, with Arctic climatic conditions, is found in the presence of recent Arctic marine shells occurring in terraces at an elevation of from 100 to 150 feet above tide. In the old crystalline region, much farther south, the terracing is said to extend to 300 feet above tide. The extent of the more recent uplift is not known, since the retreat of glaciers, the inundation of ancient dwelling-sites, and the records of tide-gauges point to present downward movement observable within historical time.

I

The Species, the Sex, and the Individual

Part II.

I HAVE been discussing characters that are related to sexual courtship, but among characters confined to one sex there are others which are connected with other actions. For instance, there are structural peculiarities which are only employed in combat. Among the most highly developed of these are the antlers of stags. It cannot be disputed that these are the special, apparently the only weapons of the stag, and that there is no stag in a state of nature which does not regularly follow the practice of duelling without any variation in the arms employed. But those who consider that the evolution of the antlers is sufficiently explained by the constant victory and survival of the stags which have them most developed, leave out of view important problems:—Firstly, why do the antlers only begin to develop when the stag becomes mature; Secondly, why are they renewed every summer, and drop off again in spring? In relation to these problems it is at least significant that the males only fight when they begin to breed, and, when mature, only in the breeding season, which is limited to the autumnal months. As the fighting of stags is fierce and frequent, it is quite possible that the irritation due to butting with the forehead was the exciting cause in the beginning, and has been ever since, of the remarkable outgrowth of bony tissue which forms the antlers. If this were so, it would be physiologically intelligible that when the stimulation ceases at the end of the butting season and the circulation becomes less active, the bone should cease to grow, should become dry and brittle, and then the antlers should either drop off of their own accord, or be intentionally broken off by the stag. Next season a renewal of the fighting would cause a renewal of the growth. My theory is, that stimulations periodically repeated, physiologically cause periodical phenomena of growth, and that these rhythmical processes of growth repeated in successive generations ultimately become hereditary. It is, or has been, a current belief that effects so caused are not inherited; but such inheritance has not yet been proved to be impossible. I am only attempting to show that the facts seem to lead inductively to the conclusion that structural evolution has

been largely determined by the direct influence of stimulation on growth.

In the reindeer, and in the bovine animals, the horns are developed in both sexes, and appear at an earlier age; those in the reindeer, however, are also periodically shed, and consist of bare bone, while those of the Bovidae are permanent and are encased in cornified skin. I do not know whether the young males and the females of the reindeer fight, or whether there is any other special habit in them to explain the development of their horns, but in the bovine animals it might be suggested that the same stimulus of butting is applied less violently and not with the same regular periodicity, and therefore has led to more permanent growth.

A brief consideration of the third kind of structural differences is now to be undertaken, namely, differences in the structure of the same individual at different periods of life. This is, in some respects, the most important of the three kinds I have defined, for it is inseparably connected with the other two. We cannot investigate the origin and cause of the differences between kinships, or between members of the same species, without studying the transformations of the individual, for these differences arise as alterations in the development of the individual.

Now the embryos of the higher vertebrates all exhibit certain characters in common, in the presence of gill-arches and gill-slits, and in the origin of the limbs as bud-like outgrowths. The great embryologist of the beginning of this century, Von Baer, whose studies were directed principally to the higher vertebrates, formulated the generalisation that animals of different classes resembled each other closely in the earlier stages of their development, and diverged more and more as they progressed toward their final form. This remains true of the higher classes of vertebrates,—reptiles, birds, and mammals. When the doctrine of evolution became paramount, and it was seen that the comparative anatomy of the higher vertebrates obviously pointed to their common derivation from ancestors which were essentially fishes, the resemblance and the structure of the embryos were attributed to the retention in these embryos of the essential characters of the fish. The generalisation of Von Baer was therefore changed into another, to wit, that in development the individual passed successively through the stages of its ancestors to arrive at its present final condition. Haeckel gave great publicity to this doctrine, calling it the biogenetic law, and formulating it in the terms that ontogeny, or the development of the individual, is a repetition of phylogeny, or the evolution of the race. The late Professor Milnes Marshall still further popularised and established the principle by embodying it in another phrase, namely, that the

individual in its development is compelled to climb its own genealogical tree.

A more comprehensive and more accurate study of the facts of development throughout the animal kingdom has shown that this law is by no means universal. It is true in regard to a certain number of facts in the development of the higher vertebrates, but it is not the whole truth about them, and it is contradicted by a great many other facts even in the development of reptiles, birds, and mammals. For example, snakes are characterised by the absence of limbs. In some snakes rudiments of the hind limbs are present in the adult condition, but in no snake has any trace of the fore limbs been discovered in any embryonic stage. Yet we cannot doubt that the ancestors of snakes possessed two pairs of limbs. Again, there can be no doubt that the wing of the bird has been evolved from a limb with five digits like that of many reptiles, but the wing contains only three digits, and the most complete embryological investigation has only succeeded in discovering small and very doubtful rudiments of the lost two. The ancestors of birds had teeth, but no trace of teeth has been found in the embryo. In the horse again there are traces of the second and fourth metacarpals and metatarsals in the limbs in the adult, but examination of the development has shown that only the merest vestiges of the second and fourth digits are ever formed, and of the first and fifth none at all.

Balfour has expressed the general result of observation in the statement that ancestral stages are liable to be omitted from embryonic development by abbreviation, and to be obscured or replaced by new characters in free larval development. He also suggested that the retention of the branchial arches and clefts in the embryo of higher vertebrates was due to the fact that these structures were functional in the larval stage of the amphibian ancestors of these vertebrates after they had become rudimentary in the adults, and that the limbs in snakes had completely disappeared because there was no such advantage in their retention at a particular stage.

Mr Sedgwick has lately elaborated this last suggestion into the general theory that ancestral characters are only retained in the embryo when the ancestral condition was once a larval condition which has more recently become embryonic, in consequence of the retention of the larva in the egg or within the body of the mother until after its metamorphosis.

These are numerous instances of the truth of Mr Sedgwick's theory, but it is not a general theory of individual development. The general theory will be found to correspond to that which I have indicated in the case of the structural differences between groups of species and between individual types in the same species.

The general truth is that when the habits and conditions are different at different periods of the individual life, then, and to a proportionate degree, will the structure of the individual be different during those periods. It may be said that this is merely another way of stating the principle of adaptation as the result of natural selection, the most advantageous variations being selected at each stage of life separately. But my contention is that, if there is not sufficient evidence of the occurrence, apart from the influence of habits and conditions, of the variations necessary to explain the other two kinds of difference, still less have we proof that the changes in the structure of the individual at different periods of life have been independent of the direct influence of the changed conditions. To my mind the phenomena of metamorphosis can only be explained on the principle that the different conditions acting on the individual at different periods of its life give rise to and determine the direction of the modifications which characterise the successive stages of the individual structure.

This matter again can only be studied in actual instances. The most familiar case is that of the frog and other Amphibia. We can have no doubt that the air-breathing Amphibia were evolved from fishes, though we may not be able to say exactly what kind of fishes. We have, however, various transitional or intermediate forms in the lower Amphibia and in the Dipnoi or lung-fishes, which breathe air to some extent. Now, how can we conceive the conversion of a single individual fish into an air-breathing creature, apart from the change of conditions, the breathing of air? It is true that the blood can and does secrete gases, oxygen and other, into a closed air-bladder, but the structural arrangements connected with the action of lungs, cannot be conceived apart from the respiration of atmospheric air. We know of plenty of cases in which, the water being scarce or foul, fish have become capable of breathing air, in one way or another, but we have no evidence of the occurrence of variations in adult life tending towards air-breathing structures in fishes which are never exposed to the air. We do not find them, for instance, in fishes that live on the sea-bottom or in the ocean abysses. When the fish is exposed to the air at a late stage of its life, then its structure undergoes modification, first into a lung-fish breathing both air and water, or into an amphibian that retains its gills throughout life. Afterwards such a form spreads into places where water is still scarcer, and it becomes still more modified, so as to breathe air altogether, and to crawl about on land.

But, at the same time, the young aquatic stage or larva is being modified. If we suppose that the tadpole resembles the ancestor of the frog, it follows that that ancestor was destitute of paired limbs

and of fin-rays, and that the terrestrial form of limb, transversely jointed into three segments and divided at the extremity into five digits, was not evolved from the fin of a fish, but was a new organ. Such a view is very improbable and by no means inevitable. It is much more reasonable to suppose that the terrestrial limb was evolved by the modification of the fin of a fish. The tadpole has lost its limbs, because in its short life their use has become diminished. It does not sustain itself in the water, but fixes itself to plants by means of its suckers, and moves from one place to another by violent strokes of its tail. Its habits have been almost as much altered as those of the frog, and its structure has been determined by its habits. Thus from an ancestral fish has been evolved a creature passing by a well marked metamorphosis from a larval aquatic stage to an adult terrestrial stage, and in each of these stages it has become very different from its ancestors.

The original reptiles were derived from the Amphibia by a change in the character of their eggs, which acquired large yolks and were enclosed in tough shells. Within the shell the larva was retained, never being set free in the water, and thus for the first time terrestrial vertebrates became entirely independent of a liquid medium. The embryo in the latter evolution of terrestrial vertebrates has undergone various modifications, but the condition in which we find it at the present day is the original larval condition of the amphibian ancestor, except so far as it has been modified by the conditions of development within the egg-shell or the uterus. Thus the course of development in the higher vertebrate is not to be explained by the law of recapitulation, according to which transient embryonic stages represent ancestral structures, but by the fact that the embryonic stage is a larval stage which passes through its metamorphosis before hatching or birth. The larval stage and the metamorphosis were originally determined by the temporary conditions of life in the individual, and the persistence of larval characters in the embryo is due to the fact that there has been nothing in the conditions of embryonic life to change them.

Let us turn now to another instance, namely, the transformation of the flat-fishes. Perhaps it will be thought that there can be no excuse for throwing doubt upon the accepted doctrine that the larva of these fish swimming upright in the water with an eye on each side of its head, repeats in individual development the conditions of the ancestor. But a more careful study of the facts shows that this doctrine is erroneous, or at least only a partial truth, and it must be modified to agree with the state of knowledge at the present time. A brief summary of the facts will be sufficient to prove this.

The flounder when first hatched is a minute larva not quite $\frac{1}{8}$th in. in length. The right and left sides are perfectly similar to

one another, and it swims vertically in the water. But it has no fin-rays and no bones, a continuous fin-membrane passes along the edge of the back round the end of the tail. The conversion of this larval form into the fully developed flounder takes place when it is from two to three months old, and about half an inch long. When the bones and fin-rays begin to develop, the left eye rises first to the edge of the head, and then passes completely over to the right side. At the same time the little fish begins to lie on its side on the ground, and loses the power of sustaining itself in the water. With slight differences in details, the development and metamorphosis of other species of flat-fish are similar. The early condition of the flat-fish therefore is not that of any fully developed fish at all, but of a fish larva without bones or fin-rays. It is in all respects similar to the larvae of other marine fishes, for instance, to that of the mackerel or that of the cod. When the bones begin to develop the eye begins to become asymmetrical, and we have not the ancestor but the flat-fish. We do not know at present whether the elongated fins along the dorsal and ventral edges had the same form in the ancestor, and we have reason to believe they had not so great an extent, yet they are developed directly, not by gradual increase. The true reading of the matter therefore is, not that the ancestral condition is repeated, but that the larval condition of the ancestor is retained, because the larva is still hatched and still lives in the same way; but the structure after metamorphosis is different because the adult fish has acquired different habits. On the theory of natural selection we must suppose that those individuals have been selected whose eyes were most symmetrical in the larval stage, and most asymmetrical in the adult condition. But we have no evidence that among symmetrical fishes individuals occasionally occur in which one eye moves up towards the edge of the head during growth. Even if slight variations of such a character were proved to exist, it would be difficult to believe that they would be great enough to make any difference to the fate of the individuals possessing them when the fish took to lying on the ground. The theory of independent variation and selection as applied to flat-fishes is unsupported by evidence, while the conclusion that the metamorphosis of these fishes is the direct result of the change of conditions is in harmony with all that we know of the effect of physical conditions on individual organisms.

In these two cases, that of the frog and that of the flat-fish, the larval condition is either unmodified or less modified from the ancestral condition than the adult. But in numerous other cases the larva has been modified in adaptation to new conditions while the adult has remained nearly the same. This is particularly conspicuous in many insects. I will not discuss at length the question of

the tracheal gills of aquatic larvae, for, although they are probably secondary adaptations in the larva, there are some who regard them as representing an ancestral series of organs from some of which the wings were derived. But if this be the case, the entire absence of wings and tracheal gills in the terrestrial larvae shows that the latter by no means recapitulate the ancestral history. Again, if the legs on the abdomen of the caterpillar behind the three pairs of thoracic legs are in any way related to the abdominal appendages of the ancestor, it is all the more certain that the maggots of the flies or of the ants, bees, and wasps, having no legs at all, cannot resemble the ancestor. In such cases the structure of the larva corresponds to its mode of life, and is much more different from any possible ancestor than is the adult. The individual does not here climb its own genealogical tree, unless it may be said to begin at the top and climb downwards. As for the origin of the modifications in the young stages, we have no evidence that their appearance was independent of the conditions; the fact that the special structure only lasts as long as the special larval habits last, suggests strongly enough the direct dependence of the modifications on the conditions of life.

To sum up the argument of which I have attempted to give an outline, its main points are these. Selection assumes the occurrence of variations: the variations must either be similarly indefinite and promiscuous in all cases, or they must be different in different cases —that is, in different species, different sexes, different stages of life. If they are different in different cases, then selection is a very unimportant matter, for the chief questions are evidently what are the differences and what made them different. To deny that the variations have always been different in different cases is to deny the most evident facts: such denial might be possible when we consider only the difference between species, but is impossible when we study the differences between the sexes in the same species and between different stages in the same individual. In all cases the variations correspond to differences in habits and mode of life, and in many cases are of the same kind as the changes known to be produced in the individual by special stimulation or special activity of organs: this is true of many and probably of all cases of adaptation. The general conclusion is that adaptation is not produced indirectly by the selection from indefinite variations, but directly by the influence of stimulation in modifying the growth of the parts or organs of the body.

J. T. CUNNINGHAM.

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II

Bees and the Development of Flowers

DARWIN and Dr Russel Wallace maintain that the bright colours of flowers are due to insects, and this view has, till recently, been accepted by most biologists. But difficulties become apparent as soon as the methods of insect workers are closely investigated. The result of such investigations has been that some naturalists (among them Mr G. W. Bulman, writing in *Natural Science*, Aug. 1897) have come to the conclusion that the colours of flowers have arisen quite independently of insects and that they have yet to be accounted for on Darwinian principles.

A priori, if it be granted that the Darwinian hypothesis affords a satisfactory explanation of other phenomena of the animal and vegetable worlds, it seems unlikely that it should leave the colours of flowers unexplained. Moreover, flowers that are invariably fertilised without insect aid are almost all of them dull and inconspicuous. The young cones of the larch are an exception. In these the colour may, possibly, be looked upon as a by-product of the physiological activity of the plant. The more striking blossoms, elaborate in form and coloration, cannot possibly be mere by-products.

The difficulty of explaining the colours of flowers, though by no means insuperable, is very real. Insects can never produce a new species unless during each journey from and back to the hive they keep to the same sort of flower. That they show a remarkable constancy is undeniable. When at work upon dandelions they will not wander to a neighbouring narcissus. In thus keeping to flowers of the same make, they are consulting their own interest: they can extract the honey with greater speed than if they wandered to flowers of a different build. Frequent practice at the same exercise produces great dexterity of limb and proboscis, and the work goes merrily on. There is but little of the tentative buzzing and reconnoitring that is unavoidable when a bee is investigating an unfamiliar flower. And thus it is to a bee's own interest not to transfer pollen to a flower of a different genus or of different family. But she is often tempted to go from one variety to another, or to a closely allied species, and she does so without scruple. Thus it is just where her constancy might seem most needed that it breaks down. When new varieties are arising, the operation of bees comes

in, to swamp them, if possible, by inter-crossing, and so prevent them from developing into species.

How bees fail as species-makers, may be seen from the following examples. In a field of buttercups there are often two species in blossom side by side, *Ranunculus bulbosus* and *R. acris.* The former begins to blossom a good deal earlier than the latter, but the flowering times of the two overlap. If you watch a bee among these, she will often for a time keep to one species. *R. acris* stands a good deal higher and, owing to this, she will for a while perhaps pass over *R. bulbosus.* But before long she will often change her level and busy herself with the lower-growing species. In a bed of mixed polyanthus flowers she may often be seen going from one colour to another, heedless of the claims of polytypic evolution. The same thing takes place when she is busy upon rhododendrons and columbines of slightly different or even widely different tints. These instances of infidelity to colour or species I select because I have recently observed them. There is no doubt that they overthrow the theory that insects by their constancy have been makers of new species. It must be owned that bees, in spite of their great reputation, dating from the days of Aristotle, are great blunderers. Still I cannot but believe that to bees and other insects are due all the brilliant colours of wild flowers. The transference of pollen from variety to variety is an undoubted fact. But what if it produces no effect? A number of French botanists, wishing to prove that evolution was a myth, have made experiments,[1] during a number of years, showing that even varieties distinguished by what appear the most trifling differences, are inter-sterile. This discovery is in reality no blow to evolution, but by the irony of fate it comes in very opportunely to help the Darwinian theory. The inconstancy or defective constancy of bees is of no consequence, since most varieties and, possibly, all species of wild plants are inter-sterile or, if they are not absolutely inter-sterile, their own pollen is prepotent, so that when two pollens are put on one flower, that which represents its own variety or species alone takes effect. In addition to all experiments made by botanists, we have those made by the bees themselves: they are constantly doing their best to inter-cross species and varieties, and we know that the species and varieties remain distinct.

To account, then, for the colours of flowers we have the proved colour-sense of bees—Sir John Lubbock has tested it by experiment: they are attracted by brilliant blossoms and, therefore, it has been to the interest of plants, in order to obtain cross-fertilisation, to produce conspicuous flowers. Every variation in the direction of

[1] An account of these experiments may be found in Romanes' *Darwin and after Darwin*, vol. iii.

increased brilliancy has been of advantage. But the bees for whose favours the plants were bidding would have ruined all, had not the plants on their side had a very marked characteristic: infertility with varieties of the same species, or with other species of the same genus. The bees have constancy enough to ensure frequent cross-fertilisation between plant and plant within one variety or species: but, intent upon their own business, they take no trouble to put barriers between new varieties. This work is done by the plants themselves. The varieties, at any rate all the large number that have survived, have kept themselves apart. Through this inter-sterility the bees have been gardeners who had each variety and each species in an isolated garden, and for whom each new variety as it arose proceeded to isolate itself without any trouble on their part. They with their colour-sense and the plants with their preference for pollen from one of their own species, or even for pollen from one of their own variety have, working together, given us all the colours, shapes, and scents of flowers.

F. W. Headley.

Haileybury College.

III

The Eskers of Ireland

Part II.

THE various theories which have been advanced to account for eskers may, I think, be reduced to two classes—

The first class will contain those held by geologists who, while differing in matters of detail, agree that the eskers result from marine agency—tides and currents.

The second class will contain those propounded by writers who see their way to dispense with marine action, and who find in the agency of glacier-ice the origin of all manner of drift deposits.

The Neptunists—a term which will serve to indicate the upholders of the marine agency theory—are well represented by Jukes and Kinahan, who have the advantage, as regards the eskers, of minute practical acquaintance with the geology of Ireland.

The Glacialists are represented by Hummel and Geikie—the former belonging to the Geological Survey of Sweden, and the latter to the Geological Survey of Scotland.

I begin with J. Beete Jukes, who was for many years, prior to his death in 1869, connected with the Geological Survey of Ireland. In his opinion the 'ridges' "seem to have been formed by the piling action of two opposing currents, or to have been heaped up in the eddy at the margin of the currents running in different directions."

But it is in this 'piling-up action' that the difficulty lies. Irregular heaps or mounds of sand, gravel, or shingle may have been got together in this way, although it would not be so easy to account for the stratification observable in the true eskers. But when we come to consider the long narrow ridges of the railway embankment type the difficulty becomes really formidable. I have no doubt that Jukes is correct in saying that "many of the eskers were perhaps similar to harbour-bars in their mode of formation, and may be directly related in this way to the valleys running into the neighbouring hills, which must, of course, have formed bays or harbours during some part of the last slow rising of the land above the sea."

In Geikie's edition of "Jukes' Manual" of Geology, there is a note on p. 712, citing, as an excellent example of an old 'harbour-bar,'

the case of the Seven Churches, Co. Wicklow: "All the ruins are on a bank of drift, stretching across the main valley, and formed partly of the detritus from that valley, but chiefly perhaps from the other steeper and narrower valley, which must at one time have emptied its drainage into the old harbour just above this point, and brought down the detritus, of which the tidal currents formed the bar."

There are, I have no doubt, drift formations in other places, which may have originated after the manner of the 'harbour-bar.' But we must bear in mind the well-marked difference in form of these accumulations and the esker proper.

"Others, however," continues Jukes, "especially those numerous ones which run in various directions all over the great central plain of Ireland, can only have been formed in the open sea by the action of different currents, as that sea became shallow in consequence of the elevation of its bed." Now, as it appears to me, this statement would go further if we could suppose the bottom of this shallow sea already thickly strewn with the prepared materials—the sand, gravel, clay, and rock fragments. As the waters would become shallow, these deposits would more and more come within reach of the surface currents, and, doubtless, would in many places become heaped up in mounds or ridges of some kind. But I am not sure that this is exactly what Jukes intends to lay down.

The 'counter-current' theory has been worked out with boldness and clearness by Mr G. H. Kinahan, in his interesting "Manual of the Geology of Ireland," chapter xiv. In this work there is no uncertainty like that just mentioned. The author includes the eskers among "the moraine drifts that are undoubtedly post-glacial." These drifts he divides into periods corresponding with alleged pauses in sea depression "at or about the 300 feet contour line," "at or about the 100 feet contour line," etc.

The margin of the esker sea, he explains, "will be found in places on the hills sometimes as a shelf cut in the sides, at other times as a beach accumulation." But, as Geikie has shown in the case of the Parallel Roads of Glenroy, near Fort William, in Scotland, the notches and old beach lines may have resulted from fresh water dammed up in the mountain valleys by the obstruction of great glaciers in the plains adjoining. At one time Geikie held the same opinion as Kinahan regarding the marine origin of the eskers. "But," he writes, "having since seen reason for believing that the sea has had no share in the formation of the Scotch gravel ridges, which are in the same category as the Irish eskers, I now look upon the latter as having been heaped up principally by the action of sub-glacial waters during the final melting of the confluent glaciers." This goes a long way towards reducing the esker sea to a myth.

In some places on the low ground the esker sea drifts (as Kinahan terms them) are spread out in undulating sheets, as in the County of Kildare. The famous Curragh may be cited as a tabular deposit of such materials. Now all these deposits point in the clearest manner to 'the rushing of great waters'; and if we only consider the thaw of such an ice-sheet, as all admit to have existed, we can have perhaps floods and rushing waters enough for any purpose, without calling in the aid of subsidence beneath present sea-level.

Mr Kinahan does not, however, forget that the formation of the eskers is a disputed point; but he thinks it probable that they are modifications of the banks and shoals which accumulate at (1) the colliding, and (2) the dividing of the flow-tide currents of the esker sea, similar to those that are found in the seas around Great Britain and Ireland at the present day. He examines three contemporaneous instances of the 'colliding' of currents giving origin to bank formations:

(*a*) In the Irish Sea, in the vicinity of the Isle of Man, there is a meeting of the north and south flow-tide waves, or a 'Head of tide.' Where the tidal currents meet they neutralise, forming a mass of currentless water that simply rises and falls, depositing silt and other materials.

(*b*) There is a 'Head of tide' in the Straits of Dover; and

(*c*) In the German Ocean, between Norfolk and Holland.

But in the Straits of Dover and in the German Ocean the meeting is not precisely a case of 'colliding,' as the currents pass for some distance; and at their edges, or their junctions, long banks of gravel and shingle accumulate.

It is also found, says Kinahan, that long banks of gravel and shingle may form at the dividing and splitting up of the flow-tide currents. We have here some resemblance between currents at sea and rivers on land. Just as the river-flow, dividing at the outlets, makes a deposit which may eventually become a delta, so does the ocean-current where it parts produce an accumulation varying in its constitution, extent and form with the nature and supply of materials affected by the moving mass of waters.

As an illustration Kinahan cites the following: From Greenore Point (Co. Wexford) a main current runs northward up the Irish Sea, while secondary currents break off into Wexford Bay; and at the junction of these currents with the main one there are long banks between Greenore Head and Wicklow Head. Similar results, he adds, must have occurred in the esker sea. He applies his theory in this way:

The flow-tide wave entering at Galway must have sent a main current eastward to the coast between Drogheda and Dublin.

Southward of this current there would be a bay, somewhat similarly circumstanced to Wexford Bay, off which banks would form between Galway Bay and Dublin, that is in the line of country occupied by the principal eskers.

That the author was himself sensible of the difficulty already alluded to, is clear from the words above quoted: "The typical eskers are very unlike shoals." By the conflict, or the separation, of flow-tide currents you can have shoals and flats and mud-banks, but can you have an esker like one of those already described? Let anyone put the question to himself as he stands beside one of these ridges, or walks its road-like top, for only in such circumstances can the difficulty be fully realized.

Kinahan, however, goes further, and explains how subsequent denudation may have played a part in the modelling process.

Although it cannot be affirmed, yet it appears possible (he says) that as the sea shallowed, and the shoals and banks became 'awash,' the current should have the power of changing the massive banks into narrow ridges, for at the half-tide or 'awash,' portions of banks, or in the shallow places where two currents collide, there are esker-like ridges as St Patrick's Bridge between Kilmore and the Saltees Co. Wexford.

But with all this before me I am unable to account to my own satisfaction for the form of the esker proper. It is not easy to conceive how by any process of marine or aërial denudation great massive banks could be attenuated to the slender figure of an esker running for miles like an artificial earthwork. One may ask how the denuding forces could waste all but the back-bone, and yet spare the latter which was, after all, no more likely to resist erosion than the rest of the drift matter.

In the first edition of his Manual, Mr Kinahan attached particular importance to the 'Head of tide' origin of banks, etc. But at a later period he abandoned this in favour of what I have sketched as the 'Cross-current theory.' He believes that, allowing the marine origin of eskers, the various details and complications of the drift formations of the central plain of Ireland could be explained by the 'colliding' or meeting of the flow-tide currents branching from the main, with these coming through the straits—now valleys—in the surrounding hills, as also the different eskers to the north of the main current.

It is indeed remarkable that the great eskers of the central valley are opposite some great gap or valley which, on the hypothesis of an esker sea, was at one time a strait or channel. For example, the Parsonstown esker is opposite the great gap of Roscrea between the Slieve Bloom mountains and the mountains of North Tipperary. The East Galway groups are in relation with the open-

ing between the Silvermines and the Slieve Boughta mountains behind Portumna. Last, not least, the great group of drift ridges and hills at Esker College, Athenry, is situated near the mouth of the great strait which occupied the valley of Gort and Lough Cooter between the mountains of South Galway and those of North Clare.

Dr James Geikie remarks that Mr Kinahan was the first to point out the relation between these groups of post-glacial mounds and ridges with the openings of valleys branching off from the great central plain; and the circumstance is further noteworthy as forming (accidentally, I think) a connecting link between the two sets of theories represented by Kinahan and Geikie respectively. While Kinahan looks to the 'colliding' or dividing of flow-tide currents, Geikie finds all that is necessary in the melting away of 'confluent glaciers.' The latter regards the explanation which Mr David Hummel gives of the åsar of Sweden as applicable to the drift formations of Scotland and (as he believes) to the analogous formations of Ireland, including, of course, the eskers.

From the observations made by him on the åsar and other drifts of Sweden, Hummel concludes that the facts indicate the agency of running water, and the direct action of glacier-ice; and he comes to the conclusion that the åsar have been formed in tunnels underneath the dissolving ice by running water introduced through crevasses, etc., acting on the ground moraines of the great confluent glaciers which covered Sweden during the glacial period. Geikie adopts a similar explanation of the kames of Scotland, and remarks that the theory to some extent resembles Mr Goodchild's view of the origin of drift deposits in general.

Dr N. O. Holst has proposed another explanation of åsar, which he supposes to have been deposited in superficial channels licked out of the ice-sheet by the water derived from the melting of the inland ice. The materials were, as he believes, derived from the melting ice in which they had lain embedded. Geikie rejects the explanation for these reasons:—

(1) Because åsar, eskers, and ridgy kames are not so continuous as they must have been had they been formed in superficial river channels; and

(2) Because we have no reason to believe that the ice of the old extinct glaciers was more thickly charged with debris than the present ice-sheets of arctic and antarctic regions.

Moreover, it occurs to me that, while it is by no means impossible that morainic matter may have collected in such grooves, it is very unlikely that it would come to *terra firma* without disturbance of stratification. The groove would be narrower at bottom than at top, the reverse of what we find in the esker.

Hummel's theory, as adapted by Geikie, while open to a share

of the objections, will perhaps be found to apply to Ireland as to Scotland and Sweden; it will, at all events, be found to explain some phenomena not easily reconcilable with other theories. Take the case of the large erratics frequently perched on the top or sides of eskers and similar deposits. When I have read that such blocks had been borne by ice-rafts which, stranding on the eminences, there melted, and dropped their burdens, I have asked without being able to find an answer satisfactory to myself: How could so slender and fragile a structure survive the rude impact of an iceberg? On Hummel's hypothesis I can satisfy my own mind at least. I can understand how crevasses would wear and widen into tunnels,—how the water flow derived from the melting ice would sweep the morainic matter into these tunnels,—how the melting would vary with the season,—how the solid matter would vary from time to time with the force of the water,—how there would be strata dipping towards the sides which would wear away unequally at different sections,—how even on the same line of crevass we may find a well-formed ridge, a mound, or a heap of morainic matter,—and how the greater blocks would remain on the ice-surface, till let down on the surface or side of the ridge. I am not sure that we can so easily account for the roundness and smoothness of most of the pebbles as we could on the hypothesis of marine action. And I am still at a loss to know why the true esker is confined to the comparatively narrow midland zone.[1] Surely there were crevasses and morainic matter elsewhere, and the great thaw would be general and pretty nearly equal all over the area of Ireland. It is easy enough to conceive that the greater part of the products of glaciation would finally become scattered about, levelled, or, in some places furrowed, by the floods which would cover all the low-lying parts of the country with deposits of sand, gravel, and shingle. Perhaps it may yet be shown that eskers were formed in other parts of the country, but were destroyed by local glaciers of later date, or by other causes. In parts of Ulster, Co. Monaghan for instance, there is abundance of limestone gravel in hillocks and mounds. That there are no long narrow ridges at present does not, perhaps, justify the assumption that there never have been any there. The preservation of an esker is, or ought to be, not less a subject for wonder than its original formation.

I have already partly described the great drift formations at the College near Athenry, and I give some further details, which may afford additional illustration of Hummel's theory. This group consists of two great parallel ridges running westward from the

[1] Some well-formed eskers may be seen adjoining the railway between Tuam and Claremorris. The locality may, however, be regarded as a portion of the great midland plain.

college (which enclose on two sides the lawn or playground), and a series of immense mounds or hills to the south-east, east, and north-east. These sand-hills form a miniature mountain system with valleys, and some curious bowl-shaped depressions. Seen at a little distance, in the twilight they may easily be mistaken for a veritable mountain-chain with sierra-like crest. But there is no appearance of rock, the whole consisting of drift deposits, mainly limestone gravel and sands, with a surface which in a 'dropping' (*i.e.* rainy) season supports a fairly good sheep pasture, and forms burrowing ground for myriads of rabbits. Apart from these ridges and sand-hills the country around is flat, and less than a century ago must have been almost entirely covered with deep bog, but most of this has been cut away. There are some outliers, and one long ridge, partly levelled and obliterated, may be traced along the Kingsland road as far as the outskirts of the town of Athenry, at one time the Anglo-Norman capital of the province of Connaught.

Having spent a number of years in this peculiar locality, I have often considered how by any known agency of tide or current these ridges and sand-hills could be shaped as they are. While some features could be accounted for by marine action, others could not be brought within range. How could the 'flow-tide' heap up two great parallel ridges within a stone's cast of each other? If it is within the function of the tides to accomplish this, I am afraid we don't quite understand the question of ways and means. By Hummel's theory we get over the difficulty. There is remarkable parallelism among the åsar of the Lake Mälar district. And if there were no such difficulty in the way of the marine theory, there is a rather formidable one as regards the heaping up of a ridge so high and steep-sided in proportion to width at top.

The eastern end of the more northern ridge has been cut away to make room for some of the buildings, and in this way is made a very good section, which, although partly obscured by a wall and by detritus at the base, affords the best view of the internal structure of an esker that I have anywhere seen. This section shows a curious alternation of sand and gravel beds with an occasional 'leaf' of clay, or rather lime-clay paste, but the dip is not quite so steep as the sides. Owing to the percolation of rain-water, there is matrix of calcareous matter which serves to bind the whole into a tolerably compact mass, which, however, readily yields to the pick-axe, and the loosened materials when screened, serve to mix with mortar for building. Over the sides and top there is, however, a deposit of aërial drift, in which the rabbits can work their way. Formerly, the peat closely surrounded both ridges. I have heard it stated that there is peat underneath the gravel. But I know that in the summer of 1897 a pump was sunk within two yards of the section

I have described, and to the depth of twenty-five feet, without any indication of peat. The boring passed through beds similar to those of the ridge, but horizontal, and of closer grain. No boulders were met with until the water-bearing stratum had been tapped, and then some large ones were encountered.

In the sand-hill group, the few openings I have seen showed less of the stratified arrangement, with a greater number of rounded pebbles, exemplifying, I think, the passage of the true esker into the morainic mounds, much acted upon by denudation.

Are we then to take it as a settled matter that the esker sea is all a myth?

I hardly think so. Even Geikie admits the probability of an epoch of depression, and mentions that "gravel beds with marine shells have been traced in Ireland up to a height of 1235 feet on Montpelier Hill." Again, how are we to account for the presence of large blocks of the red porphyritic granite of western Galway on the Slieve Bloom mountains? This granite, as Jukes remarks, "is easily recognisable inasmuch as it contains hornblende instead of mica, and has large crystals of pinkish felspar, and is therefore porphyritic." How could these erratics be borne to their present situation except on rafts of floating ice? It is known that blocks of stone will sometimes rise through the glacier to its surface. But in such cases the erratics do not rise above the level of their origin; they merely describe a plane of less incline than the upper surface of the glacier. It may be contended that the period of depression did not synchronize with the formation of the eskers, and if so, we have new difficulties to meet. It may be that the eskers were not all formed at one time or in one way, and that most of the theories apply to a certain extent, while no one theory yet propounded, is comprehensive enough to cover all the ground, or clear enough to explain all the circumstances. I have no intention and no ambition to attempt a new solution. But I have frequently been struck by what appears to me the marked resemblance between some eskers and certain phenomena in progress round the shores of Galway Bay.

In his essay on "The Arenaceous Rocks of Ireland" (*Proc. R. Soc. Dublin,* n.s., vol. v., p. 507, 1887), Kinahan describes a curious spit of conglomerate at Lisbellaw, Co. Fermanagh, which he believes to have been formed in Silurian times after the manner of the Chesil Bank. "In Lyme Bay the flow tide current runs from the westward of Portland Bill which acts as a groyne; Chesil Bank or Beach becomes coarser and larger as it is followed east, till it forms a massive heap of shingle to the west of the Bill; but eastward of the Bill, in Weymouth Bay, there are finer accumulations. In Silurian times similar forces were at work in the neighbourhood of Lisbellaw."

And similar forces, I should say, were at work in the 'esker' sea, as they are at the present day in Galway Bay, and must have been at work on some parts of the margin of the sea in every geological age. Mr Kinahan makes incidental reference to the ridge from Co. Wexford to the Saltees. Within three miles of the mouth of the Corrib River, on which Galway stands, there are examples of what I take leave to call Chesil Bank formation in progress at the present hour, some to the east and some to the west of the outlet.

East of the town, and separated from it by the inner bay known as Lough Atalia (crossed by the railway) is the promontory on which stands Renmore Military Barracks. Off this headland is Hare Island, a rather remarkable fragment of the boulder-clay drift which appears in cliffs just opposite and in other places around the Bay. At low water this island is connected with the mainland by a natural causeway, about half a mile in length, of so regular construction that it would, at first sight, appear almost as the work of man. At high tides this causeway or bar is covered under water deep enough to float a small schooner. At the land end it joins other ridges more of the 'harbour-bar' character, running to right and to left along shore, and cutting off lagoons from the Bay, the bars being above the reach of all but the highest tides. "Who made this road?" I asked an old man residing in the hamlet hard by. "The tide," he answered; "the bank has grown out from the island, and is still growing." The island divides the flow-tide, and the shingle and sand are ridged up very much as Kinahan's theory lays down. More curious still is the great loop which the ridge makes at the island, forming a deep pond or loch of over an acre in extent, the surface of the enclosed water being, when I saw it, at ebb-tide on 24th May 1898, fully ten feet above the level of the surrounding waters. Near the famous Claddagh, a great bank of shingle has cut off many acres inshore, flooded only at high tides through a gap in the 'bar.' At the western end of this ridge there is also a fragment of boulder-clay. Again, about a mile to the west of the beautiful sea-side suburb, Salthill, is a conspicuous promontory of the boulder-clay known as Mount Gentian (now the Golf Links). Off this headland is another island fragment of the clay-drift; and this too has been joined to the shore by a long narrow causeway of shingle which stands clear of the highest tides, and may be traced for a mile in the direction of Salthill promenade, in the form of a 'harbour-bar,' cutting off a considerable space of old beach now converted into grazing ground..

The two marine causeways here described present some striking resemblance to many of the inland ridges I have seen. There is resemblance in contour, of slope, of proportion, of bedding; and if

the 'causeways' have a preponderance of pebble and shingle, that is owing to the circumstance that the shore in question is thickly strewn with granitic boulders.

It is not necessary, I think, to point out that I do not advance these remarks as a solution of the esker question. At the same time, I submit that the circumstances are deserving of some attention when the question is as to the way or ways in which the typical eskers have been formed. However much we may feel inclined to prefer the explanations offered by Hummel and Geikie, we cannot, I fancy, yet afford to discard in its entirety the principle so ably worked out by Kinahan. That there is a ridge-forming power in the flow-tide action of the great ocean—when the current meets some object sufficient to part it in the shallower waters near the coast—we can see for ourselves in such instances as I have just cited. That the eskers, properly so-called, are mainly confined to the zone which may be regarded as a continuation of Galway Bay is, to my mind, a circumstance more easily reconcilable with Kinahan's than with Geikie's later theory. Yet I do not regard this aspect of the question as by any means fatal to the principle propounded by the latter. If we have not typical eskers outside the midland zone we have at least their ruins, and these in plenty, in the counties of Monaghan, Tyrone, Fermanagh, Mayo, etc.; that is, we have such heaps, mounds, and flats as would be produced by the partial dispersion of a true esker ridge. In whatever way we may suppose the ridge to have been produced we can see that there were many chances against survival in the perfect form. That so many have withstood 'the shocks of time and chance' is rather wonderful; for hardly less curious or less puzzling than their origin is the question as to the conservation of so many eskers in Ireland and of corresponding ridges in other countries.

THOMAS FITZPATRICK.

ST IGNATIUS' COLLEGE,
GALWAY.

597.55(53.1)
639.(53.1)

IV

The Grey Mullet Fishery in Japan

THERE are three known species of *Mugil* in Japan, namely, *M. cephalotus*, *M. haematocheilus*, and *M. joyneri*.

The first species is known by the name of *Shirome* (white eyed), *Bora*, *Nayoshi*, &c., and while immature *Subashiri*, *Oboko*, *Ina*, &c., according to different stages of development. The second species is called *Akamĕ* (red eyed), *Shukuchi*, &c.; and the third species, *Meina*. The first species is abundant along the whole coast of the main island or Hondo, and is captured throughout the whole year. In summer we find innumerable fry of this fish in brackish water, and often in fresh water too. In spring the fish migrate in shoals towards shallow water, and remain scattered there during the whole summer, while in autumn they assemble together and form shoals again and migrate along the coast. Then as winter approaches they gradually seek warmer and deeper water, and pass the cold season in rather a dormant state. They are also cultured in brackish ponds, and sometimes in canals round such rice fields as are near the sea.. Their average length is a little more than a foot.

The second species, *Mugil haematocheilus*, is abundant in the southern district, and is caught in autumn when it comes in large shoals towards the coast for the purpose of spawning. Roes of this fish are salted and dried, and are highly esteemed as a delicacy. This fish attains a larger size (about two feet) than the preceding species.

The third species, *M. joyneri*, is not abundant, and consequently is not important economically.

The average annual catch of the grey mullet is about four hundred thousand *yen* in value, about £40,000. There are very diverse methods for its capture. Sweep-seines, dip-nets, pound-nets, hand-nets, stop-nets, set-nets, drift-nets, cast-nets, and drift-lines are the chief apparatus used for the purpose. These appliances are chiefly used in shallow waters, not more than ten fathoms in depth, as the fish are mostly found in these parts.

The mullet, especially *Mugil cephalotus*, is said to be frightened by sound and light. Moreover, it is said that the fish hate the presence of oily substances, so that they turn their route of migra-

tion from the place where such are found. Therefore, at the fishing ground, fishermen do not throw away any oily substance out of the boat; moreover, they keep very quiet, uttering no word.

The sweep seine.—There are many kinds of this seine, different in structure as well as in the method of using. I shall describe some nets of special interest.

The double floats seine (Fig. 1).—This is simply a long, narrow net, of which the height is greatest in the middle and decreases gradually towards either end. The length of the rope at both upper and lower margins of the net is 600 feet. This net has a very long and narrow netting added to its upper margin like a roof, to prevent the fish from leaping out of it. The narrow netting has its own floats on the free margin, hence the seine has two rows of floats. This special structure makes the net adapted for the capture of the mullet. When

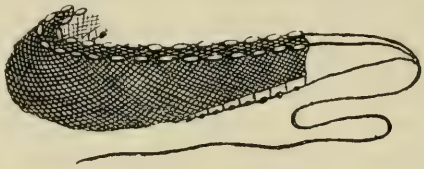


Fig. 1.

the net is in use, a small boat constantly attends to the roof-like netting to make it keep the right position and not to collapse; this is done by binding two or three floats together, as the seine is gradually drawn towards the shore and the two wings approach each other more and more. This seine is used by a boat manned by seven men to encircle a shoal of fish, and it is done under directions from a watch-tower on shore. When the seine is completely put out, it is dragged in by about thirty men on shore. This net can be used only in flat sandy shores.

The sweep-seine fishery with a sunken rope.—In certain parts of Central Japan a peculiar method is employed. During the night a big straw rope is sunk to the bottom of the sea at right angles to the coast line, and fishermen wait silently on land for the approach of fish with a boat ready in which a sweep seine is loaded. It is said that when a shoal of fish approaches, some fish are frightened by the sunken rope and leap out of water. The fishermen hearing the sound of the leaping fish, jump into the boat and hastily encircle the seine.

The dip net (Fig. 2).—Of this net also there are many kinds. It is used generally on rocky ground. It is rectangular or square in shape, and is mostly of a large size (some nets measure about 240 feet by 120 feet). The net is sunk at a convenient place in the route of migration of the fish. In some cases the net is

entirely sunk under the surface of water, while in other cases only one side is sunk. In rocky districts where the sea is generally deep, the mullet swims very close inshore. The net is often set in a bay having a slight curvative, because such a place is more convenient than an open coast or a coast deeply indented. In the former case the sea is more rough, while in the latter case the fish does not come often. The movement of the fish is observed from towers on land, and the communication between them is made by signals. When the fish come on the net, the sunken side or sides are immediately

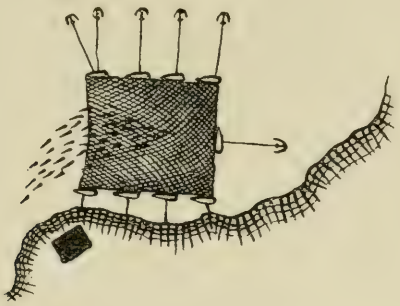


FIG. 2.

raised by hauling ropes which are fastened to the sides of the net. Ropes are held by anchored boats, and are hauled by a signal from the watch-tower nearest to the net. Such a dip-net is sometimes accompanied with two long wings, which are used to encircle the fish and force them to go on to the sunken net as well as to prevent them from escaping. As the quick performance of work is necessary for this fishery, numerous boats and men are generally employed.

The pound-net.—The apparatus (Fig. 3) next to be described is not a proper pound-net, because fish do not come into the net by themselves, moreover there is no special device to prevent the

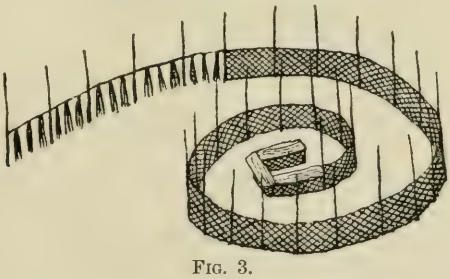


FIG. 3.

fish which enter the net from going out again. The net is spiral in shape. It is supported by bamboo sticks, stuck into ground, and consists of three different parts—outer, middle, and inner or central.

A straw rope with a few stems of straw, put between strands of the rope and suspended at regular intervals, forms the outer part. The end of these straw stems nearly touch the ground. The middle part forms the greater portion of the pound-net, and is made of a netting which is stretched between bamboo sticks to form a barrier. The central part is short. Its essential portion consists of three wooden boards, placed in the shape of the letter U. The space between the boards and the ground is cautiously closed by a netting. To catch fish, three or four persons row a small boat very swiftly, and at the same time drive scattered fish by beating the surface of water with oars or poles. When the boat comes near the end of the net, whence it cannot go further, a man comes out of the boat and continues the driving in the same way, wading towards the centre of the net. The fish are at last compelled to leap to clear the barrier, and are caught by the boards. This net is used in a shallow ground at the outlet of a brackish lake. The mouth of the net faces the outlet of the lake.

There is another kind of pound-net (fig. 4) used in a shallow bay where there is a great difference in the height of water between the flood and ebb tides. It is a very long net, set by means of poles, parallel to the coast line, and bent towards the coast at both ends. There is a fold, or a series of pockets, which runs through the whole

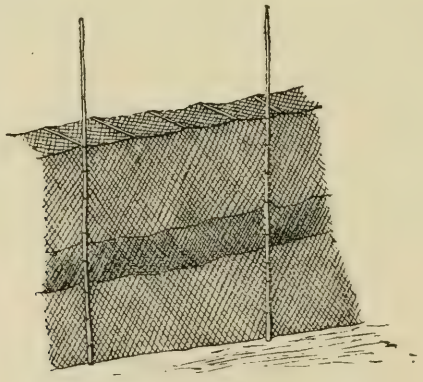


Fig. 4.

length of the net at its middle part. The mouth of the pocket opens towards the coast. When the tide recedes, the upper part of the net is bent backward to form a roof. The roof is supported by a series of floats. Fish are entangled in the pockets, or caught on the roof-like part.

The hand net (fig. 5).—This is a simple apparatus, but the method of using it is somewhat interesting. It is trapezoid in shape; its upper and lower margins are strengthened by ropes, and the two slanting sides by bamboo poles. This net is used on the shallow

sandy shore of a bay. The two bamboo sticks are held by two men wading in the water, or by two boats. Thus the net is expanded between them, special care being taken not to leave an open space between the lower margin of the net and the ground. The net is held obliquely against current. While the net is thus prepared, two boats row swiftly and drive fish towards the net by means of a long

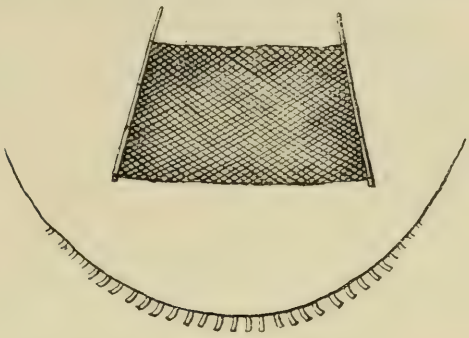


FIG. 5.

scare-cord held between them. When the cord touches the net, the latter is quickly raised out of water to bail out the fish. The scare-cord is made of hemp, and is about 900 ft. in length. It is provided with numerous thin, small, rectangular pieces of wood (ea. 1 ft. by 2 ins.). They are slightly curved, and make a noise, and disturb the surface of the water very much when they are drawn quickly.

The stop net.—This is chiefly used in harbours to stop escape of fish, either by encircling a shoal of fish, or by closing the mouth of the harbour. This is generally accomplished with set-nets or gill-nets. For the benefit of this fishery, a certain district is closed

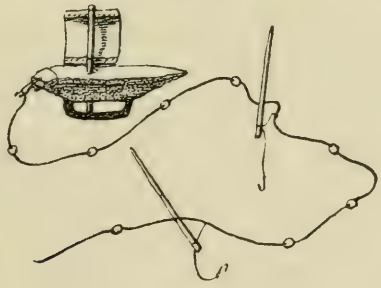


FIG. 6.

to fishermen, and sometimes even to the anchorage of boats, during a certain season. To allure fish to such a place, bran and mud are mixed together and made into balls, which are distributed over the ground.

The drift line (fig. 6).—This is used from a boat in a brackish lake. The line is made of hairs from horse tails, twisted together

to the thickness of about one line, and 120 to 140 yards in total length. Hooks, about ten in number, are attached to the line by means of short snoods. Moreover, there are many small, round floats attached to the line. The snood is provided with a long float made of wood near the point of attachment of the snood to the line. When a fish is caught on a hook, the long float belonging to the hook stands out of the surface of water. The hooks are baited with earthworms. At the distal end of the line a boat-shaped float with a sail is tied, by means of which the line is sent far from the boat. This apparatus is used only for sport.

K. KISHINOUYE.

IMPERIAL FISHERIES BUREAU,
TOKIO, JAPAN.

590.6

V

The Zoological Congress

THE fourth meeting of the International Zoological Congress was held at Cambridge between the 22nd and 27th of August, under the presidency of Sir John Lubbock. The attendance was more than twice as great as at any previous meeting, and the whole Congress must be regarded as an unqualified success.

The plan of work comprised a discussion on some problem of general zoological interest in the morning, a series of sectional meetings for the consideration of more technical matters in the afternoon, with garden-party or reception in the evening.

The proceedings opened on Monday evening with a reception by the Mayor of Cambridge to the members of the two Congresses of Zoology and Physiology, both of which were sitting during the same week. On Tuesday morning Sir John Lubbock began the work of the Congress by delivering his Presidential Address. Following the precedents set by the previous Presidents, Milne Edwards, Kapnist and Jentink, Sir John Lubbock confined his address to a welcome to the members, some interesting remarks on the great field of work still open to zoologists, and reference to the flourishing school of zoology which has its seat at Cambridge. The general report of the Congress was read by the treasurer, and Dr van Hoek announced some reforms in the postal charges for natural history specimens that were to be granted. Zoological nomenclature was brought up in reference to a report of a committee on that eternal question; but, to the general relief, any discussion was cleverly shelved until the next Congress.

The two principal meetings of the Congress were held on Wednesday and Thursday morning. At the former, Professor Delages and Mr Minchin opened a discussion on the "Position of Sponges in the Animal Kingdom." The fight soon resolved itself into a long-range skirmish between the old men and the young. Delages and Minchin both held that the inversion of the embryonic layers in the sponge precludes the reference of that group to the Coelenterata, though whether it is to be ranked as a separate group, or as directly descended from the Choanoflagellata seemed less certain. Haeckel and Schulze both clung to the Coelenterate theory. Vossmaer declined to express an opinion, and Saville Kent reaffirmed his belief in the

affinity of the sponges with Choanoflagellata. The general trend of the discussion certainly seemed to favour the newer view.

On Wednesday morning the problem for discussion was the Origin of the Mammalia, opened by Professors Seeley and Osborn, and continued by Marsh, Sedgwick, Hubrecht, and Haeckel. The main question round which the discussion ranged was whether the mammals had descended from the reptiles, instead of from the amphibia, as is now generally taught in text-books in accordance with Huxley's teaching. Professor Seeley's speech was a clear comparison of the osteology of the anomodont reptiles with that of mammals. He showed that all the supposed mammalian characters are also found among the extinct anomodonts, which have completely broken down the distinction between reptiles and mammals. Professor Osborn began by deducing the probable characters of the ancestral mammal from certain general considerations. He agreed with Seeley as to the origin of mammals from reptiles, but preferred to regard the former as descended from a third, as yet undiscovered, group of anomodonts, instead of going back to the Devonian or Silurian for some common ancestor of mammals and anomodonts. Marsh expressed no positive opinion on the main question, but insisted on the fact that the reptiles and mammals are still separated by four important osteological characters. He adduced strong reasons for considering that the resemblances between mammals and reptiles adduced by Professor Seeley may be explained as a case of parallel adaptation, the same characters having been independently acquired. Sedgwick made an effective speech protesting that embryological evidence will not help in the solution of such a remote question as that under discussion, as it gives no indication of the polydactyle stage of the ancestral horse, the toothed stage of birds, or the limbed stage of snakes, though the existence of those stages is not doubted by any one. Professor Hubrecht did his best to defend embryology from this emphatic statement of its limitations, and Haeckel, strangely conservative, still upheld the origin of all placental mammals from the marsupials. But this view was generally scouted, and the discussion seemed to strengthen the case for the reptilian ancestry of the mammals.

The principal feature in the Friday morning's proceedings was Haeckel's discourse on the Descent of Man. The previous day's discussion anticipated much that he intended to say, so he did not read his paper, and until that is printed it is not possible to estimate the scientific value of his contribution. His speech was popular, and Haeckel received an ovation at its close.

The afternoon meetings were devoted to papers of more technical character and exhibitions in the museum. The papers were of very unequal value. A few were advertisements of forthcoming works,

by the exhibition of sample plates. That comparatively little of the time of the Congress was wasted in this way is no doubt due to the courage with which the secretaries had rejected the more obviously useless papers. Among the exhibits those of Mr Graham Kerr, Mr Stanley Gardiner, Mr Willey, and Mr Rousselet are especially worthy of mention.

On Saturday morning a business meeting was held, at which it was arranged that the next meeting should be in Germany during 1901. The Congress then adjourned to London for visits to the Zoological Gardens and British Museum (Natural History), where on the Saturday evening Sir John Lubbock gave a reception. On Monday a large number of members went to Tring to see Mr Rothschild's magnificent museum, and on Tuesday a smaller party under the guidance of Mr Lydekker inspected the Duke of Bedford's collection of deer at Woburn Abbey.

In such a varied course of proceedings it is difficult to draw any general conclusions as to results. Thanks to the skilful plans of the secretaries, Professor Jeffrey Bell, Mr Sedgwick, and Mr Bourne, and the tactful arrangements of the secretaries of the local reception committee, Dr Harmer and Mr Shipley, the Congress was held without a hitch. The opinion was generally expressed that the scientific standard of the papers was much higher than usual. The discussions were especially enjoyed, and will no doubt henceforth be regarded as a chief feature of future meetings. From the proceedings three impressions seem to have been generally felt. First, the scanty attendance of entomologists and ornithologists may indicate the increasing separation of those branches of zoological work. The ornithologists have a congress to themselves, and the entomologists also will not improbably start one, to settle their own difficulties without the interference of specialists in other subjects. Secondly, the greater importance attached to palaeontology, and the increasing distrust of embryology as a guide in phylogeny, were shown repeatedly. Thirdly, there seemed a feeling of boredom with the interminable question of zoological nomenclature, and a certain determination to refuse to follow rigid rules when they lead to absurdities, and to trust more in the future to common sense.

VI

James Hall

IT has been given to few men to serve for upwards of sixty years on the official staff of a public department in the Old World; and probably the late Dr Hall's sixty-two years of service in the Geological Survey of New York State is unique in American annals.

James Hall was born on the 12th September 1811, at Hingham, an old-fashioned New England town near Boston, which claims the oldest occupied church in America. Early in the century it was little more than a fishing village, and Hall received his scientific education in the Rensselaer School at Troy, which has since grown into the Troy Polytechnic Institute. He graduated there in 1832, when he was appointed assistant professor of chemistry and natural science. In 1836 he was transferred to the professorship of geology, and in the same year was appointed to one of the posts of assistant geologist on the newly established Geological Survey of New York State. In the following year he was promoted to the rank of State Geologist, and began field work during the same year. From 1838 until 1843 he was engaged on the survey of the western part of the state, and began the study of the recession of the Niagara Falls, to which he acted as Lyell's guide in 1841. In 1843 he wrote his last field report, and was appointed the State Palaeontologist. Under his supervision systematic fossil collecting was undertaken in the rich palaeozoic faunas of New York, resulting in the formation of the magnificent collections in the Albany Museum. The faunas were described in "The Palaeontology of New York," of which thirteen huge imperial quarto volumes were issued between 1847 and 1894 at the estimated cost of about a million dollars. In addition to writing and editing this great work, Hall contributed a long series of reports to the annual volumes of the "*Regents Reports of the New York State Museum*," and a number of other papers in the usual scientific serials. In 1848 he was elected a Foreign Member of the Geological Society, and ten years later received from the same society its Wollaston Medal, of which at his death he was the senior recipient by no less than eleven years, while the third medallist in seniority was elected twenty-three years later. In 1855 he was offered the post of palaeontologist to the Canadian Geological Survey, with a promise of the reversion of Sir William Logan's position of Director. He declined the offer, but worked out the collection of Canadian graptolites, which were described in a report on "The Graptolites of the Quebec Group," published in 1865.

In 1855 Hall accepted the post of State Geologist of Iowa.

That office and the similar position for the state of Wisconsin, which Hall received in 1857, he was able to hold without affecting his position in the New York service. His work in connection with these surveys increased his knowledge of the geology of the Mississippi basin; and he was able to make important additions to the geology of the western states, as he was entrusted by the Federal Government with the description of the collections made by many of the expeditions and surveys in that region. Thus he described the fossils collected by the Fremont expedition, the collections of the Pacific Railway Survey, the Cretaceous fossils obtained by the Mexican Boundary Commission, and wrote a report on the geology and palaeontology of the basin of the Great Salt Lake of Utah from materials brought back by Lieutenant Stansbury.

In 1856 Hall was elected President of the American Association for the Advancement of Science. At its meeting in Montreal in the following year he gave his famous address, in which was first definitely expounded the theory that the elevation of mountain chains is due to the deposition of sedimentary deposits, and that the direction of the mountain chains are determined by lines along which the thickest accumulation has taken place.

But this and similar incursions into the domain of physical geology only temporarily distracted Hall from palaeontographical work to which his inexhaustible energies were mainly devoted. His additions to the materials of invertebrate palaeontology are probably greater than those of any other man. The number of important new genera founded by Hall is enormous. The roll of Hall's new genera and species was by 1858 so long, that Colonel Portlock, the President of the Geological Society whose duty it was to present Hall with the Wollaston Medal, felt bound to qualify his patronizing commendations by a warning that he was himself "prone to hesitate respecting new species when closely allied to previously known species." But the work which Colonel Portlock appeared to disparage is now recognised as Hall's most permanent title to fame. As a note in the *Geological Magazine* reminds us, palaeontology is indebted to Hall for the following important genera: among the graptolites there are *Callograptus*, *Dicranograptus* and *Phyllograptus*; among the corals, *Coelophyllum*, *Heliophyllum* and *Streptelasma*; among the Pelmatozoa, *Calceocrinus*, *Heterocrinus*, *Dendrocrinus*, *Glyptaster*, *Glyptocrinus* and *Hemicystis*; there is the star-fish *Palaeaster*, and the echinid *Lepidechinus*; the additions to the Monticuliporoids and Bryozoa are very numerous, including *Favistella*, *Callopora*, *Bactropora*, and *Trematopora*; and among the Trilobites are *Pleuronotus*, *Bathynotus*, *Mesothyra* and *Ptychaspis*. His Memoir on North American *Eurypterida*, *Pterygoti* and *Ceratiocaris* (1871),

is one of the most valuable contributions to these forms of Arthropods. And Hall not only described fossils systematically; he carefully studied their anatomy and the microscopic structure of their tissues, a work in which the magnificent preservation of the North American palaeozoic fossils gave him exceptional opportunities.

In 1872 Hall visited England to attend the meeting of the British Association at Brighton, and read a paper "On the Clinton, Niagara, and Upper Helderberg Formations in the United States." Four years later he helped to found the International Congress of Geologists, an institution in which he always took a keen interest. In fact only last year, in spite of his 86 years of age, he visited Russia to attend the last meeting of the Congress and subsequently accompanied it in a fatiguing excursion through the Ural Mountains. In 1884 Hall was elected a corresponding member of the Paris Academy of Sciences.

During the last few years of his life Hall was greatly worried by the friction with the literary departments of the New York State service with which his own was associated. Originally the scientific departments were under the control of the library and literary branches of the service, which regarded the great cost of the scientific departments with disfavour. At length in 1893 Hall succeeded in getting a bill through the New York legislature, which secured his freedom from literary control. An attack was then made on his private character. Mr Melvil Dewey, the state librarian, charged Hall with having sold for 65,000 dollars a collection of fossils which he said were really the property of the State. A legislative committee investigated the charges, Dr Hall was triumphantly acquitted, and the New York Geological Survey has continued its work in peace.

Hall's indomitable energy, unfailing courtesy and bright good humour rendered him an universal favourite in American scientific circles. His humour was keen, and though sometimes cynical, never marred by any suspicion of unkindness. For example, his wife was a Roman Catholic and had at one time converted Hall to her views. Hall used to attribute his abandonment of Romanism to the breaking of a pulley chain, whereby there were simultaneously smashed to shivers one of his favourite fossils and his faith in providence. After this Hall's relations with his wife were not so sympathetic; for she appeared a little jealous of his devotion to his scientific work. So he built a house for his wife in his park, and they lived together for years on terms of friendly neighbourship. She died some years ago, and Hall keenly felt her loss. He himself passed away rather suddenly on August 7th, at a quiet resort in the White Mountains, where he had gone for his usual summer's rest.

VII

Animal Intelligence as an Experimental Study

THE investigation of the problems suggested by the observable phenomena of instinct and intelligence in animals is passing—we may now say has passed,—into the experimental stage. The collection of anecdotes, useful enough for preparing the ground and (as Time's irony has shown) for enabling one to perceive the insecurity of any such basis for reliable conclusions, has had its day. It is realized by serious students that, not only for the interpretation but also for the observation of the phenomena, if they are to serve the ends of science, some real training and discipline in psychology are essential. Dog-stories and cat-stories though often full of subtle humour and though not infrequently revealing an affectionate and imaginative nature, serve rather to tickle the fancy than to appeal to the rational faculties. It is not on such foundations, nor with such materials, that a science of comparative psychology can be securely built. Observations *ad hoc* by an investigator trained *ad hoc*, will always carry weight. But the casual jottings of well meaning though uninstructed people serve rather to check than to forward the diffusion of exact knowledge.

Mr E. L. Thorndike in a monograph on "Animal Intelligence" published as a supplement to the *Psychological Review* (June 1898) has approached his subject in the right way, as one full of difficult problems to be grasped, faced, and if possible solved, and has furnished an experimental basis, narrow perhaps, but capable of further extension for the conclusions that he draws. I have briefly noticed his work elsewhere (*Nature*, July 14th, 1898); but I regard it as of sufficient importance to justify a more extended presentation and consideration here.

The subjects (one might, alas! almost say victims) of Mr Thorndike's experiments—or those to which the exigences of space compel us to confine our attention—were thirteen kittens or cats from three to eighteen months old. His method of investigation shall be stated in his own words.

"After considerable preliminary observation of animals' behaviour under various conditions, I chose for my general method one which, simple as it is, possesses several other marked advantages besides those which accompany experiment of any sort. It was merely to put animals when hungry in enclosures from which they could escape by some simple act, such as pulling at a loop of cord,

pressing a lever, or stepping on a platform. The animal was put in the enclosure, food was left outside in sight, and his actions observed. Besides recording his general behaviour, special notice was taken of how he succeeded in doing the necessary act (in case he did succeed), and a record was kept of the time that he was in the box before performing the successful pull, or clawing, or bite. This was repeated until the animal had formed a perfect association between the sense-impression of the interior of that box and the impulse leading to the successful movement. When the association was thus perfect, the time taken to escape was, of course, practically constant and very short.

"If, on the other hand, after a certain time the animal did not succeed, he was taken out, but *not fed*. If, after a sufficient number of trials, he failed to get out, the case was recorded as one of complete failure. Enough different sorts of methods of escape were tried to make it fairly sure that association in general, not association of a particular sort of impulse, was being studied. Enough animals were taken with each box or pen to make it sure that the results were not due to individual peculiarities. None of the animals used had any previous acquaintance with any of the mechanical contrivances by which the doors were opened. So far as possible the animals were kept in a uniform state of hunger, which was practically utter hunger."

To Mr Thorndike's monograph we must refer those who desire detailed information as to apparatus and procedure. It must here suffice to state that the box-cages employed were rudely constructed of wooden laths, and formed cramped prisons about twenty inches long by fifteen broad and twelve high. Nine contained such simple mechanisms as Mr Thorndike describes in the passage above quoted. When a loop or cord was pulled, a button turned, or a lever depressed, the door fell open. In another, pressure on the door as well as depression of a thumb-latch was required. In one cage two simple acts on the part of the kitten were necessary, pulling a cord and pushing aside a piece of board; and in yet others three acts were requisite. In those boxes from which escape was more difficult a few of the cats failed to get out. The times occupied in thoroughly learning the trick of the box by those who were successful are plotted in a series of curves, the essential feature of which is the graphic expression of a gradual diminution in the time interval between imprisonment and escape in successive trials. In some cases the cats were set free from a box when they (1) licked themselves or (2) scratched themselves.

Mr Thorndike comments on the results of his experiments as follows:—

"When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening; it claws and bites at the bars or wire; it thrusts its paws out through any opening and claws at everything it reaches; it continues its efforts when it strikes anything loose and shaky: it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigour with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly. . . . The cat that is clawing all over the box in her impulsive struggle

will probably claw the string or loop or button so as to open the door. And gradually all the other non-successful impulses will be stamped out and the particular impulse leading to the successful act will be stamped in by the resulting pleasure, until, after many trials, the cat will, when put in the box, immediately claw the button or loop in a definite way. . . . Starting, then, with its store of instinctive impulses, the cat hits upon the successful movement, and gradually associates it with the sense-impression of the interior of the box until the connection is perfect, so that it performs the act as soon as confronted with the sense-impression. . . . Previous experience makes a difference in the quickness with which the cat forms the associations. After getting out of six or eight boxes by different sorts of acts the cat's general tendency to claw at loose objects within the box is strengthened and its tendency to squeeze through holes and bite bars is weakened; accordingly it will learn associations along the general line of the old more quickly. Associations between licking or scratching and escape are similarly established, and there was a noticeable tendency to diminish the act until it becomes a mere vestige of a lick or scratch. After the cat gets so that it performs the act soon after being put in, it begins to do it less and less vigorously. The licking degenerates into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch, the cat waves its paw up and down rapidly for an instant."

These experiments confirm the conclusion to which I have been led by my own observations that the method of animal intelligence is to profit by chance success and to build upon fortunate items of experience casually hit upon and not foreseen. I need not here repeat cases already published, such as the opening of a gate on the part of my fox terrier by lifting the latch, a trick he certainly learnt by this method; but I may very briefly describe one or two further observations not yet recorded. I have watched my dog's behaviour when a solid indiarubber ball was thrown towards a wall standing at right angles to its course. At first he followed it right up to the wall and then back as it rebounded. So long as it travelled with such velocity as to be only just ahead of him he pursued the same course. But when it was thrown more violently, so as to meet him on the rebound as he ran towards the wall, he learnt that he was thus able to seize it as it came towards him. And, profiting by the incidental experience thus gained, he acquired the habit—though for long with some uncertainty of reaction—by slowing off when the object of his pursuit reached the wall so as to wait its rebound. Again, when the ball was thrown so as to rebound at a wide angle from a surface, at first,—when the velocity was such as to keep it just ahead of him,—he followed its course. But when the velocity was increased he learnt to take a short cut along the third side of a triangle, so as to catch the object at some distance from the wall. A third series of experiments were made where an angle was formed by the meeting of two surfaces at right angles. One side of the angle, the left, was dealt with for a day or two. At first the ball was directly followed. Then a short cut was taken to meet its deflected course. On the fourth day this method was well estab-

lished. On the fifth the ball was thrown so as to strike the other or right side of the angle and thus be deflected in the opposite direction. The dog followed the old course (the short cut to the left) and was completely non-plussed, searching that side and not finding the ball for eleven minutes. On repeating the experiment thrice similar results were that day obtained. On the following day the ball was thrown just ahead of him so as to strike to the right of the angle and was followed and caught. This course was pursued for three days, and he then learnt to take a short cut to the right. On the next day the ball was sent, as at first, to the left and the dog was again non-plussed. I have not yet succeeded in getting him to associate a given difference of initial direction with a resultant difference of deflection. And since these words were written the dear little fellow has died. No doubt it will be said by some fortunate possessor of a particularly rational dog that my fox terrier was a fool. Let him experiment and record the stages of progress, remembering that a rational being will quickly and surely pierce to the heart of the mystery.

I may here mention that whenever searching for a ball of which he had lost sight in the road he would run along the gutter first on one side and then on the other. A friend who was walking with me one day regarded this as a clear case of rational inference. "The dog knows," he said, "the effects of the convex curvature of the road as well as we do." I am convinced, however (having watched his ways from a puppy), that this method of search was gradually established on a basis of practical experience. No logical inference on his part is necessary for the interpretation of the facts; and we should not assume its presence unless the evidence compels us to do so.

Such experiments carried out on a different method give results in line with Mr Thorndike's. The conditions are more natural which I regard as in some respects an advantage. But we need experiments on different methods,—the more the better,—and if the results they furnish are in accord, their correctness will be rendered the more probable. I hope, however, that Mr Thorndike will devise further experiments in which (1) the conditions shall be somewhat less strained and straitened, while the subjects are in a more normal state of equanimity (cannot "utter hunger" be avoided?), and (2) there shall be more opportunity for the exercise of rational judgment, supposing the faculty to exist. To establish the absence of foresight in the procedure of the cats, it is surely necessary so to arrange matters that the connections are clearly open—nay even obvious—to the eye of reason. It appears to me that this consideration has not weighed sufficiently with Mr Thorndike.

A number of interesting experiments were made with a view to testing the influence, if any, of imitation.

"A box was arranged with two compartments separated by a wire screen. The larger of these had a front of wooden bars with a door which fell open when a string stretched across the top was bitten or clawed down. The smaller was closed by boards on three sides and by the wire screen on the fourth. Through the screen a cat within could see the one to be imitated pull the string, go out through the door thus opened and eat the fish outside. When put in this compartment, the top being covered by a large box, a cat soon gave up efforts to claw through the screen, quieted down and watched more or less the proceedings going on in the other compartment. Thus this apparatus could be used to test the power of imitation. A cat who had no experience with the means of escape from the large compartment was put in the closed one ; another cat, who would do it readily, was allowed to go through the performance of pulling the string, going out, and eating the fish. Record was made of the number of times he did so and of the number of times the imitator had his eyes clearly fixed on him. . . . After the imitatee had done the thing a number of times, the other was put in the big compartment alone, and the time it took him before pulling the string was noted and his general behaviour closely observed. If he failed in 5 or 10 or 15 minutes to do so, he was released and not fed. This entire experiment was repeated a number of times. From the times taken by the imitator to escape and from observation of the way that he did it, we can decide whether imitation played any part. . . . No one, I am sure, who had seen the behaviour of the cats would have claimed that their conduct was at all influenced by what they had seen. When they did hit the string the act looked just like the accidental success of the ordinary association experiment. But, besides these personal observations, we have in the impersonal time-records sufficient proofs of the absence of imitation. It therefore seems sure that we should give up imitation as an *a priori* explanation of any novel intelligent performance. To say that a dog who opens a gate, for instance, need not have reasoned it out if he had seen another dog do the same thing, is to offer instead of one false explanation another equally false. Imitation in any form is too doubtful a factor to be presupposed without evidence."

Mr Thorndike is of opinion that monkeys are probably imitative in a sense that cats and dogs are not. But this is not at present substantiated by analogous experiments. I trust that he will submit it to this test.

As Mr Thorndike himself well observes, it is necessary clearly to differentiate the various meanings which are intended when the word "imitation" is used. The most elementary form of imitation—that, of which, I believe, we find abundant evidence in the procedure of animals—is where the performance of a simple act by one individual suggests the performance of a similar act by another. This is the "plastic limitation" of Professor Mark Baldwin, and is analogous to mimicry as a biological phenomenon in this respect; it is imitative from the observer's point of view but does not imply intentional imitation on the part of the performer. Conscious and purposive imitation involves faculties of a high order; and I am not prepared to accept its existence in animals,

even the Primates, without more conclusive evidence than is at present forthcoming. But unconscious imitation of the follow-my-leader type (the outcome of a direct suggestion) is a factor of prime importance alike in animal and in human life. And of this Mr Thorndike's experiments do not offer any disproof. A cat with no experience of the means of escape sees another perform a certain act and learns nothing from the experience. This no doubt proves that the cat had not in any sense grasped the problem to be put before it; and shows that when placed in similar difficulties it did not go back upon its previous merely observational experience (if such it can be called). But the previous experiments have already gone far to disprove the rationality of the cat—have at any rate thrown the onus of proof on the upholders of the alternative hypothesis. The whole gist of the chance experience interpretation of animal behaviour is that there must be such chance experience to build on. The cat cannot gain this by looking on, never so intently, unless it be provided with a rational, as well as a sensory eye. But the act of pulling the string is not of the type that can reasonably be regarded as likely to afford a follow-my-leader suggestion. It has been reached by the gradual elimination of many failures; it is a differentiated act, and one therefore so far removed from the ordinary procedure of kittens. In all this I think Mr Thorndike will agree. But his statements might well lead readers of his work to suppose that he denied this influence of suggestion. When he lays it down that "to say that a dog who opens a gate, for instance, need not have reasoned it out if he had seen another dog do the same thing, is to offer instead of one false explanation another equally false," he is, I think, open to misconstruction. Puppies at a gate do most certainly in some cases (I speak from observation) follow the lead in an unmistakable manner, and unquestionably profit by the suggestive behaviour of one of their number. To contend that they imitate with conscious intent would be quite another matter.

A series of experiments were made to ascertain whether instruction (in the form of putting the animal through the procedure requisite for a given act) was in any degree helpful. The conclusion is that such instruction has no influence. Those who have had experience in teaching animals to perform tricks will probably agree here—though some trainers give expression to a different opinion. It is, however, essential carefully to distinguish between showing an animal how a trick is done, and furnishing useful accessory stimuli (such as the occasional taps of the trainer's whip) which temporarily enter into the association complex. If the latter be eliminated the practice of trainers, I believe, bears out the general result of the experiments. Mr Thorndike never

succeeded in getting an animal to change its way of doing a thing for his. Nor was I, after repeated trials, able to modify the way in which my dog lifted the latch of the gate. He did it with the back of his head. I could not get him to do it (more gracefully) with his muzzle.

It is not my purpose to discuss Mr Thorndike's psychological analysis of the procedure of the cats. I think he is a little disposed to emphasise the points in which he and I differ—though in many cases the difference is more apparent than real. But in truth we agree on more points than I care here to enumerate. I fully concur in the opinion that what we have to deal with in animal intelligence is a sequence of conscious situations; that it is only through analysis of mental complexes that we separate and isolate free ideas; that man can do this, and that the animal in all probability can not. I am absolutely at one with him in the belief that the method of animal intelligence to profit by chance experience without rational foresight, and that unless such experience be individually acquired, the data essential for intelligent progress are absent. While in our attempts to realise the general nature of animal consciousness there is a close similarity of treatment. In my *Introduction to Comparative Psychology* I devoted a good deal of space to an analysis of the psychology of skill "in order that we may infer what takes place in the minds of animals"; and I said:—"When I am playing a hard game of tennis, or when I am sailing a yacht close to the wind in a choppy sea, self does not at all tend to become focal. Hence, though I am a self-conscious being I am not always self-conscious. And presumably when I am least self-conscious, I am nearest the condition of the animal at the stage of mere sense experience. I am exhilarated with the sense of pleasurable existence, my whole being tingles with sentient life. I sense, or am aware of, my own life and consciousness, in an unusually subtle manner. Experience is vivid and continuous. Such I take it to be the condition of the conscious but not yet self-conscious animal."

I can therefore cordially endorse Mr Thorndike's conclusions as expressed in the following passages:

"One who has watched the life of a cat or dog for a month or more under test conditions, gets, or fancies he gets, a fairly definite idea of what the intellectual life of a cat or dog feels like. It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts about them or memories of how they

looked at other times, or æsthetic judgments about their beauty; one feels no ideas about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connections of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all."

And after an illustration from such a game as tennis, Mr Thorndike adds :—" Finally, the elements of the associations are not isolated. No tennis-player's stream of thought is filled with free-floating representations of any of the tens of thousands of sense-impressions or movements he has seen and made on the tennis-court. Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind."

There is much in Mr Thorndike's monograph to which there is not space to allude. He is weak in that historical sense which gives continuity to the development of scientific interpretation, but I regard his investigation as one of great promise, and believe that its further prosecution will lead to other results not less important than those which he here presents. Experimental work in this field is sorely needed; and Mr Thorndike has proved himself one who is able and willing to carry it out.

C. Lloyd Morgan.

16 Canynge Road,
Clifton, Bristol.

SOME NEW BOOKS

Fossil Plants for Students of Botany and Geology. By A. C. Seward, M.A., F.G.S. (Cambridge Natural Science Manuals.) Pp. i-xviii+1-452. With frontispiece and 111 figures. Cambridge: University Press, 1898. Price 12s.

No better indication could be given of the increased interest in the study of fossil plants than that afforded by the publication during the last few years of several text-books specially dealing with this branch of Natural History. At last it seems to be recognised that the student of recent botany must possess some knowledge of fossil botany to enable him to have a comprehensive view of his subject and equally, if not even more important is it, for the palaeobotanist to be thoroughly conversant with recent botany and especially with those groups which are more closely connected with those occurring in the fossil state. It is only when Fossil Botany has been so studied that any real advance can be made, and the present work admirably brings before us in a clear and lucid manner, the results which have been attained within the last few years from the study of fossil botany on these lines. I am certainly not one to declaim against the older workers who laboured under disadvantages which the modern student can scarcely appreciate, and who by patient work laid the foundation on which all workers must build, but by modern methods of research unknown to them, fossil botany now holds an important position in biological science, to which it has only attained within the last few years.

Only the first volume of Mr Seward's work has appeared. It begins with the lowest forms known as fossils and ends with the Sphenophyllales. Part I., consisting of six chapters, deals with matters connected generally with the study of fossil plants, while Part II., comprising chapters vii.-xi., treats of the systematic portion of the subject.

Chapter i. gives a short historical sketch, while chapter ii. deals with the Relation of Palaeobotany to Botany and Geology. A short but very concise geological history is given in chapter iii., which is quite sufficient to show the succession and chief characteristics of the various "Formations." Chapter iv., which describes various modes of Preservation of Plants as Fossils, we consider one of the most important parts in the work. It is only after much practical experience in collecting and examining fossils, that one learns how much to allow for differences in appearance, even in the same species, which are entirely due to different modes of fossilization.

Chapter v. on the "Difficulties and Sources of Error in the Determination of Fossil Plants" is also admirably written and must be carefully read by all students. It illustrates the utter absurdity of classification based on external similarities of appearance or form. I may quote one sentence which shows a good use such errors may serve in the future. "It would serve no useful purpose, and I would occupy no inconsiderable space to refer at length to the numerous mistakes which have been committed by experienced writers on the

subject of fossil plants. Laymen might find in such a list of blunders a mere comedy of errors, but the palaeobotanist must see in them serious warnings against dogmatic conclusions or expressions of opinion on imperfect data and insufficient evidence"; and it is in this spirit, not one of severe criticism, that we must study many of the earlier writers.

Nomenclature is dealt with in chapter vi., and though a text-book is not the place to treat fully this "difficult and thorny question," the general principles are laid down for the protection of the name of the original describer as the author of the species, even though circumstances demand its removal to another genus than that in which it was originally placed.

Part II., beginning at chapter vii., deals with the systematic portion of the subject. The divisions of the plant kingdom are taken in their natural sequence, beginning with the lowest and passing gradually to the highest group. The Thallophyta are therefore first considered, the various groups or genera being preceded by a short account of their recent representatives. Here are included the *Girvanella* of Nicholson and Etheridge. The supposed fossil bacilli are described in some detail, and though some minute bodies are possibly correctly included here, a great deal of uncertainty hangs over many of these so-called Bacteria.

The Algae form a difficult class. Undoubtedly most of the fossils originally described as Algae are inorganic markings and animal tracks, though a few true Algae have been found in the fossil state—even in palaeozoic rocks. Examples of tracks simulating Algoidal structures are given which will illustrate the difficulties in distinguishing between true fossils and inorganic markings. We believe Mr Seward is correct in the doubts he holds as to the vegetable nature of *Chondrites verisimilis*, Salter.

It is impossible to refer in detail to the many interesting points touched on by the author in his admirable treatment of this very difficult class of fossils, but *Nematophycus* — the *Prototaxites* of Dawson—deserves a passing notice. This plant is very fully gone into by Mr Seward, who arrives at the conclusion that "on the whole it is probably the better course to speak of *Nematophycus* as a possible ally of the brown Algae rather than as an extinct type of the Siphoneae," and this is going quite as far as our knowledge of the fossils warrants.

In chapter viii. the Bryophyta are considered. We are here treading on very difficult ground, and especially in regard to those species discovered in the older rocks, none of which seem to be altogether free from doubt.

When we reach the Pteridophyta, chapter ix., one possesses more certain data from which to form an opinion of the affinities of the various fossils described, though even here there is room for much difference of opinion.

The Equisetaceae are first described,—a sketch of *Equisetum* prefacing the study of its fossil allies, and here are met with some of the most interesting fossils one requires to consider.

The genus *Equisetum* has been recorded by several writers from Palaeozoic rocks, but Mr Seward places all these in *Equisetites*, and as

there is no absolute certainty that any of these so-called palaeozoic Equiseta are identical with the recent genus, he probably takes the right course. Among those described are *Equisetites Hemingwayi*, Kidston sp.; *Equisetites spatulatus*, Zeiller; *E. columnaris*, Brongt.; and *E. lateralis*, Phill. The consideration of *Phyllotheca* and *Schizoneura* lead us up to the Calamites—which is one of the most important groups—(for it cannot be considered a genus in any true sense of the term)—of palaeozoic times.

The study of the Calamites is introduced by a historical sketch, and this is succeeded by a description of the anatomy of the stems, leaves, roots, and cones, each of which is illustrated by good figures.

This class he treats with great skill and shows a complete mastery of a very difficult subject. He recognises three types of Calamite stems as determined from internal structure:—

Arthropitys, which is the type commonly met with in the Lancashire and Yorkshire Coal Fields.

Arthrodendron, which is very rarely met with in Britain.

Calamodendron, which hitherto has not been recorded from Britain.

The structure of the leaves is next given, after which follows a description of the Calamitic genera that are founded on impressions. These are:—*Calamocladus* (*Asterophyllites*), of which *Calamocladus equisetiformis* is fully described as a typical form. This is succeeded by a description of *Annularia*, *A. stellata* and *A. sphenophylloides* being given in full detail. The roots of Calamites are next dealt with, and with them is now associated the *Astromyelon* of Williamson.

Three types of Calamitic cones are described. These are *Calamostachys* (with which is united *Stachannularia*); *Palaeostachya* and *Macrostachya*. *Huttonia* is also referred to, but with the exception of the first two genera, little is known of their internal structure, without which their true position cannot be satisfactorily determined. Much still requires to be done in the elucidation of Calamitic cones and great care must be exercised in forming opinions as to their affinities.

The impressions and casts of stems are now considered and are classed under the three sub-genera of Weiss:—*Calamitina*, *Stylocalamites* and *Eucalamites*. The study of Calamite stems presents considerable difficulty. Many specimens which have been regarded as stems are certainly only the casts of the pith cavity, but others, such as many *Calamitina* show the outer surface of the stem. These three sub-genera are distinguished by the manner in which the branches were borne on the stems—a system of classification which can only be regarded as provisional. In very few cases can the foliage and cones be associated with their parent stem.

Archaecalamites, from the Lower Carboniferous and Devonian, forms another group, which is closely related in some of its characters to *Calamites*, but is clearly separated from that genus by several important differences in the stem and cones.

Chapter xi., which concludes the volume, is devoted to the Sphenophyllales.

As in the previous groups the structure of the stem of *Sphenophyllum* is first described. Owing to the numerous specimens of *Sphenophyllum* which have been discovered showing fine structure, the develop-

ment of the vascular bundle from its most rudimentary form to that of the fully developed stem is fully known. These are illustrated in the work before us. The structure of the stems of *Sphenophyllum insigne* and *Sphenophyllum plurifoliatum* are given in detail.

The cones of *Sphenophyllum* offer an interesting field of study. Though the vegetative system of all the species possess many points in common, the sporangia in their number and details of attachment to the bract, differ considerably in the few species of which we possess any concise knowledge. As illustrating these differences in the structure of *Sphenophyllum* cones, several species are described in detail under the generic name of *Sphenophyllostachys*. The first, *Sphenophyllostachys Dawsoni*, Will. sp., is almost certainly the cone of *Sphenophyllum cuneifolium*, Sternb. sp. In this cone each bract bears three sporangia and each sporangium is supported on a slender sporangiophore, but as all the sporangiophores arise from the same point on the bract and are placed in sequence, the sporangium most distant from the axis of the cone has a much longer sporangiophore than that next the axis. In *Sphenophyllostachys Roemeri*, Solms Laubach sp., the incurved end of each sporangiophore bears two sporangia. There is here also, probably, three concentric circles of sporangia. In *Sphenophyllum trichomatosum*, Stur, each bract seems to have borne a single sessile sporangium. It is therefore seen that although in the vegetative system, all the *Sphenophyllum* possess many common characters, in the arrangement of the sporangia, the cones show important differences. Mr Seward rejects, and we believe correctly, the idea that *Sphenophyllum* was an aquatic plant. There is absolutely no evidence in support of this view and very much against it.

Mr Seward's "Fossil Plants" is a most successful treatment of a difficult subject. All of importance is brought forward and impartially discussed and numerous references are given to the original papers consulted. The work however is not a compilation, but embodies the opinions of one who has done much good original work in Palaeophytology. Such a book has long been a desideratum, and its appearance must give a great stimulus to the study of fossil botany in Britain. Mr Seward's style is clear and concise, and the many pitfalls into which beginners are apt to stumble are clearly pointed out. We heartily congratulate the author and publishers on the completion of the first volume of "Fossil Plants," and have only to express the hope that ere long the completing volume will be issued.

A full list of the works referred to in the text is appended and the index, a most important matter, is very full. The illustrations are also good and well chosen. K.

BAU UND LEBEN UNSERER WALDBÄUME. By Dr M. Büsgen. 8vo, pp. viii + 230, with 100 figs. in the text. Fischer, Jena, 1897. Price, 6 marks.

DR BÜSGEN has produced a useful introduction to the study of forestry, which will, doubtless, find a welcome in the various forestry schools on the Continent. Naturally, much of the subject-matter is similar to that of the general text-book of botany, embracing the principles of the anatomy and physiology of plants. But the woody plant is always kept in view, and made to supply the necessary illus-

trations. The result is a book which may take the place of the elementary botanical text-book, and at the same time give the prospective forester some idea of the more special part of his work.

The scope of the volume may be gauged from a review of the headings of the fifteen chapters. Chapter i. deals with the general habit of the tree as determined by its mode of branching, a study eminently fitted for the half of the year during which the trees are leafless. In chapter ii. the causes of the tree-form are discussed, including the direct influence of the external conditions, gravity, light, and wind. The bud is the subject of the third chapter, which ends with a table in which a number of woody plants are arranged in a clavis, according to the position and external appearance of the buds. As might be expected, the grouping by no means follows the natural affinities, unlike plants being thrown together, such as mountain ash, walnut, and like (*e.g.* willow and poplar), often widely separated. The remaining chapters refer to the general structure of stem, leaf, root, and the function of individual tissues, always with special reference to the woody plant; chapter viii., for instance, is devoted to the consideration of the density and structure of various woods. The last chapter deals with the flower and fruit, and the germination of the seed. The illustrations, many of which are borrowed, are considerably below the average of the general text-book.

The Anthropology of Peru

The work of compiling bibliographies is apt to receive far less recognition than it deserves, considering the great value of such work to students, and we are glad to have the opportunity of saying a word in praise of the "Bibliography of the Anthropology of Peru," by Mr G. A. Dorsey, recently published by the Field Columbian Museum, Chicago. This should certainly prove of considerable use, since it appears to have been compiled with considerable care and much labour, and has the appearance of being a very complete list of works written upon this interesting region. That there should be omissions is inevitable, but so firm a foundation will readily bear an appendix. All the known editions of the earlier Spanish works are given, and, what is very welcome, short biographical notices of some of the early writers are appended. The arrangement is by authors, alphabetically, and in a second part, to be published later, an index by subjects and topics is promised, and will add much to the usefulness of the work. We might suggest that an appendix of abbreviated titles, chronologically arranged, would have its value, especially in the case of the contemporary writers. This bibliography was commenced in 1893 in the form of a card catalogue for the compiler's own use in his special studies, but, fortunately, Mr Dorsey was persuaded to continue and complete the work for the benefit of others. That the labour was great cannot be doubted any more than that the result was well worth the pains. "This is my first attempt at bibliography," says Mr Dorsey, "it shall be my last as well. . . ." This remark seems to echo a sigh of relief on the completion of a long and tedious piece of work, but many, to whom such bibliographies are of great value, will regret the latter part of the sentence.

Invertebrata of France

Fauna de France. Thysanoures, Myriopodes, Arachnides, Crustacés, Némathelminthes, Lophostomés, Vers, Mollusques, Polypes, Spongiaires, Protozoaires. By A. Acloque. 1664 illustrations. 18°, pp. 500. Paris: J. B. Baillière et Fils. Price 10 frs.

This is the third volume of Mr Acloque's Fauna of France, Coleoptera appearing in 1896, Orthoptera and the remainder of the insects in 1897. We are promised a fourth and last volume to contain the Vertebrata and Tunicates. We are also indebted to the author for a flora which appeared in 1894. Acloque takes his subject genus by genus, giving a brief diagnosis of each of them, then following on with the species recorded from France, he similarly gives short diagnoses and localities. The numerous figures are sketchy, but no doubt characteristic and useful for identification. There is room for a book on this plan dealing with the English fauna.

Bibliography of Mexican Geology

Bibliografia Geologica y Minera de la Republica Mexicana, formada par Rafael Aguilar y Santillan. 4°, x+158 pp. Mexico: Oficina tipografica de la Secretaria de Fomento. 1898.

Dr Rafael Aguilar deserves hearty thanks for this excellent and compendious Bibliography. No less than 1953 items in the geology of Mexico are sufficient to appal anyone who knows the weakness of our London libraries on special subjects of this kind. The list is arranged in double columns, under authors in alphabetical order, while the last eight pages are devoted to indexes of the principal localities cited and of the more important matters. It is well and clearly printed and will be a great accession to all geological libraries and students.

Bibliography of Westralian Geology

The first Bulletin of the Geological Survey of Western Australia, is devoted to a Bibliography by Mr A. Gibb Maitland. Mr Maitland, who was formerly on the Geological Survey of Queensland, has arranged this under authors, and like so many of these useful works it was originally compiled for his own convenience. Mr H. P. Woodward, his predecessor, assisted Mr Maitland with a list of works extracted from the catalogues of the British Museum. As the Bibliography contains Papers, Reports, and Maps bearing upon the Mineralogy, Mining, Geology and Palaeontology of Western Australia, we need not refer further to its importance.

Dr Chas. Davison hopes to publish, with Messrs Cornish of Birmingham, a volume on the Hereford Earthquake of December 17th, 1896, provided that a sufficient number of subscriptions be obtained to cover the cost of its production.

This earthquake was one of the most important ever recorded in this country. Though inferior to the Essex earthquake of 1884, with regard to the damage done to buildings, its disturbed area was at least twice as great, being not less than 100,000 square miles. It was felt in every English county, except the three northern ones, over the whole of Wales and the Isle of Man, and in the eastern counties of Ireland. The number of observations on which the discussion is

founded is 2902, coming from 1940 different places, and in no previous case have the observations been so detailed and interesting.

Among the facts and conclusions of scientific importance which Dr Davison claims to have established with regard to the Hereford earthquake the following may be mentioned:—The position of the centres of disturbance is determined, and also the direction and hade of the originating fault. It is shown that there were two entirely distinct centres, lying in a north-west and south-east line, and separated by a few miles, the north-west centre being the first in action by a few seconds. A series of new lines called 'isacoustic lines' (or lines of equal sound-audibility), is drawn; these throw an important light on the origin of the earthquake. Coseismal lines (or lines passing through places where the shock was felt at the same instant) are drawn for the first time with an approach to accuracy, and by their means the average velocity of the earth-wave (which, in the case of any but a very strong shock, was unknown) is determined. While the estimates of the direction of the movement in a limited area vary widely among themselves, it is nevertheless found that the average of all these directions passes through the centre.

We hope that the publication of so interesting a work will not be prevented by any backwardness on the part of British geologists.

Scraps from Serials

In the *Transactions of the Manchester Microscopical Society*, there is a paper on Botriomyces a microcosm which produces tumour of the jaw in oxen chiefly, and was formerly regarded as a malignant cancer known as osteo-sarcoma. This is by Mr Worstenholme. Mr Gillanders reviews the Hemiptera-Homoptera, and Mr Mark Sykes treats of Natural Selection in the Lepidoptera. This latter paper, which we hope to notice elsewhere, is beautifully illustrated by eight plates.

The *Proceedings of the Royal Society of Victoria* (vol. x. pt. 2) contains papers by W. S. Dun on some new Upper Silurian Corals; J. Dennant on a new Unio from the River Glenelg (*U. glenelgensis*; a much wrinkled form resembling young examples of *U. australis*); E. R. Waite on Muridae from Central Australia with two new generic names founded on species of Gould's (*Podanomalus* and *Thylacomys*); J. Dennant and Clark on the Miocene of the Gippsland Lakes area; Pritchard and Gatcliff on *Coralliophila wilsoni*, a new gasteropod from Port Philip; Baldwin Spencer on Initiation Ceremonies in the Arunta Tribe; T. S. Hall, Stylasteridae from the Victoria Tertiaries with a new genus *Deontopora*; Officer and Hogg the second part of the Geology of Coimaidai; and Ada M. Lambert on a new land Leech (*Philaemon pungens*; Blanchard, undescribed).

In the *Journal of Conchology* for July there is an interesting account of the pairing of *Limax maximus, L.*, by Mr Lionel E. Adams, fully illustrated by Dr J. W. Taylor. Observations were made on the whole performance and much information concerning the curious suspensory threads was obtained; the anatomy of the parts is also given by Mr W. M. Webb.

THE *Boletim de Museu Paraense*, October 1897, contains papers on the Simii of the New World by Hermann Meerwarth, with maps of the distribution of each genus, further notes on the geology of Brazil by Fred. Hartt, the Devonian fauna of the river Maecuru by Friedrich Katzer, notes by Dr Goldi on *Lepidosiren* and *Mesomys ecaudatus*, and J. Huber on plants of the genus *Hevea*. All the papers are in Portuguese.

THE *Memorias Sociedad "Antonio Alzate"* (vol. xi., Nos. 5-8) contain the education of the Mexican woman by Galindo y Villa, the origin of individuals by Professor Herrera, public instruction in Mexico by Torres Torija, gold in Mexico by E. Ordonez, the water supply of the city of Mexico by Dr A. Peñafiel, and seismic notes from Central and South America by Montessus de Ballore. Herrera's, Ordonez', and Montessus' papers are in French, the others in Spanish.

THE first part of Dr Arthur Willey's "Zoological Results based on material from New Britain . . . and elsewhere," contains the anatomy and development of *Peripatus novae-britanniae* by the editor, *Metaprotella sandalensis* a new Caprellid by Dr Paul Mayer, *Aipysurus annulatus* a rare marine snake by G. A. Boulenger, reports on the centipedes, millipedes, scorpions, pedipalpi and spiders by R. I. Pocock, and an account of the Phasmidae and their eggs by Dr D. Sharp.

The Geological Survey of India notify that by order of the Government the "Records" issued by their department in the months of February, May, August and November each year, ceased to be published from the 1st January 1898. Annual Reports will be issued, and papers will appear in the Memoirs.

FURTHER LITERATURE RECEIVED

ZOOLOGICAL results based on material from New Britain, New Guinea, Loyalty Islands, and elsewhere, collected during 1895-7, Arthur Willey: Cambridge Univ. Press. Composition of Maize, W. H. Wiley: U.S. Dept. Agric. Bull. 50. Geology for Beginners, W. W. Watts: Macmillan. Faune de France (Thysanoures—Protozoaires), Acloque: Baillière et Fils. Bibliografia geologica y minera de la Republica Mexicana, Rafael Aguilar: Secretaria de Fomento, Mexico. Report on West Australian Rocks, G. W. Card. Darwin Wrong, R. F. Licorish: Barbados. Studien über die Protoplasmaströmung bei den Characeen, by Georg Hoermann: Gustav Fischer. Wisconsin Geol. and Nat Hist. Survey, Bulletins i., ii. Manual of the Geology of India, Economics, ed. 2, part i., Corundum by T. H. Holland: Geol. Survey, Calcutta. History of Mankind (Macmillan), pt. 28. *Mem. Soc. Antonio Alzate*, xi., pts. 5-8. *Proc. Biol. Soc. Washington* (numerous papers by Bangs and Preble). College of Agriculture and Mechanical Arts, Mesilla Park, Annual Catalogue. Annals of the South African Museum, part 1: Trustees. On Pontobolbos, and other papers, Arthur Dendy. Additions to the fossil Flora of Queensland, J. Shirley.

Amer. Geol., Aug.; Amer. Journ. Sci., Sept.; Amer. Monthly Micro. Journ., Aug.; Amer. Nat., Aug.; Botan. Gazette, Aug.; Feuille des jeunes Nat., Sept.; Irish. Nat., Sept.; Knowledge, Sept. 1; Literary Digest, Aug. 13, 20, 27; Naturae Novit., Aug., Nos. 15 and 16; Naturalist, Sept.; Nature, Aug. 18, 25, Sept. 1, 8; Nature Notes, Sept.; Naturen, June-Aug.; Photogram, Sept.; Plant World, Aug.; Psychol. Rev., Sept.; Review of Reviews, Aug., Sept.; Revista Quin. Psich., Aug.; Revue Scient., Aug. 20, 27, Sept. 3, 10; Science, Aug. 19, 26, Sept. 2; Scientific Amer., Aug. 13, 20, 27, Sept. 3; Scot. Med. and Surg. Journ., Aug., Sept.; Scot. Geogr. Mag., Sept.; Victorian Nat., Aug.; Westminster Rev., Sept.

OBITUARIES

NICOLAS AUGUSTE POMEL

BORN 1821. DIED AUGUST 1898

THE death of Pomel removes from Algeria a distinguished mineralogist and geologist and one who had made for many years a particular and detailed study of the country and its palaeontology. Auguste Pomel commenced his geological career by writing in 1842 several papers on the geology of the Auvergne; his thirty-fourth paper (1854) was the first written on African matters, since when he has published little short of 100 papers on Northern Africa. Perhaps his best known work is his essay on the classification of the Echinoids in which he founded new genera by the score. The work was regarded with such disapproval that it was deliberately ignored by the Zoological Record, which declined to record his swarm of new generic terms; and it was denounced with much vigour by the late Professor Duncan and Mr Sladen, in a paper for which the title of "Pomelism and Crime" was suggested. His "Catalogue méthodique du mammifères tertiaires," 1853, is a wonderful book, and was undeservedly discredited by Paul Gervais and other writers.

JOSEPH CHARLES HIPPOLYTE CROSSE

BORN 1827. DIED 7TH AUGUST 1898

WE regret to record the death of this distinguished conchologist, which occurred at Vernou (Seine-et-Marne) at the age of 71 years. Mr Crosse was the editor of the *Journal de Conchyliologie*, a journal founded by Petit de la Saussaye, and continued by Paul Fischer and Bernardi until 1861, when Crosse took the place of Bernardi, and the two raised the *Journal* to one of first importance. Among Mr Crosse's chief works we may mention the Mollusca of Mexico and Guatemala in the Mission Scientifique au Mexique, and the Mollusca in Grandidier's Madagascar. He worked chiefly on exotic forms and contributed frequently to his own Journal.

FÉLIX BERNARD

BORN 1863. DIED AUGUST 1898

ANOTHER zoologist of great promise has been removed at an early age in the person of Félix Bernard, of the Paris Museum. He was best known by the series of papers on the hinge of the bivalved mollusca, which considerably advanced the study of that difficult group, and which have been noticed at length in these pages. He also wrote "Eléments de Paléontologie," 1895.

Mr G. E. Grimes, whose appointment to the Geological Survey of India we announced in October 1895, succumbed to an attack of cholera at Thayetmyo, Burma, on the 11th April last. Mr Grimes had shown great promise as a stratigrapher during the two and a half years he had been in the service, and was only twenty-six when he died.

Johan Lange, the distinguished botanist, died at Copenhagen, April 3, aged eighty. His principal works were a Manual of the Danish Flora and the last ten volumes of the Flora Danica. He was President of the Danish Botanical Society for twenty-seven years; for twenty years Director of the Copenhagen Gardens; and was also Professor of Botany at the Agricultural College there.

Among others whose deaths have been recently recorded are :—Fredrich Charles Aplin, the ornithologist, at Bodicote, aged 43; Dr E. B. Aveling, formerly assistant in physiology at Cambridge and professor of chemistry and physiology at New College, well known as a populariser of evolution and a lecturer and writer on socialism, died in London on August 4, aged 47 years; Evert Julius Bonsdorff, formerly professor of anatomy and physiology at Helsingfors University, aged 88; Luigi Balzan, the arachnologist of the University of the Ascension, Paraguay; Axel Guttbrand Blytt, professor of botany at Christiana University, on July 25, aged 54; Dr. Ernest Candèz, the coleopterologist at Glaris, near Lüttich, on June 30; Pasquale Conti, the botanist, who died at Largano after a lingering illness; Dr Devry, the naturalist and physiologist, best known for his researches into the pharmaceutical properties of quinine, at the Hague, on August 7; The Right Hon. Murray Edward Gordon Finch-Hatton, twelfth Earl of Winchelsea, well known for his agricultural interests, on September 7, aged 48; Dr D. P. Frame, veterinary surgeon and microscopist, at Kansas City, on February 25; Carlo Giacomini, professor of anatomy at Turin University on July 5; C. W. A. Hermann, the mineralogist, at New York on June 21, aged 97; Professor D. S. Kellicott, one of the best known microscopists in America, who died at the Ohio State University in April last; João Maria Moniz, the botanist, on July 11 at Funchal, aged 75; the metallurgist, Bernard Möbius, while travelling from America to Europe on May 17, aged 46; Professor Park Morrill, of the Forecast division of the Weather Bureau of the United States, died at Washington on August 8 of typhoid fever; Henri Vander Meuten, a well-known horticulturist, at Ixelles, Belgium, on July 24, aged 83; Carmelo Scinto Patti, geologist and engineer of Sicily, born January 21, 1829, died February 7, 1898; Achille Poitan, an enthusiastic naturalist of Aubervilliers and the canton of Pantin, aged 23; Dr E. Lewis Sturtevant, the agronomist, on July 30 at Framingham, Mass., aged 56; W. F. R. Suringar, professor of botany in the University of Leyden, and Director of the Gardens and Herbarium there; Major-General Robert Gosset Woodthorpe, closely associated with the geographical exploration of India, born 1844, died May 26, 1898, of whom a long obituary notice and a portrait appears in the *Journal of the Royal Geographical Society*.

NEWS

The following appointments have recently been made :—Leopold Adametz as veterinary professor at the High School for Agronomy at Vienna ; Prof. W. P. Blake, of Tucsan, Arizona, as State Geologist of Arizona ; Dr Carl Brick as assistant in the Hamburg Botanic Garden ; Rudolf Beyer as honorary professor of botany in Berlin ; Friedrich Blochmann, of Rostock, as professor of zoology at Tübingen University ; Friedrich Moritz Brauer as director of the zoological collections in the Hof-Museum, Vienna ; Dr W. T. Brigham as director of the Berenice Panahi Bishop Museum at Honolulu ; Dr Frederick E. Clements as reader in botany in the University of Nebraska ; Hermann Dingler as professor of botany at the Institute of Forestry at Aschaffenburg ; Dr O. V. Darbishire as demonstrator in Botany at Owens College, Manchester ; Benj. M. Duggar and Dr Elias I. Durand as readers in botany at Cornell University ; Dr Adrian Fiori as private docent in botany, Padua University ; Albert Heischmann, of Erlangen, as professor of zoology at the University there, in the room of Dr Selenka ; Dr Karl Fritsch as director of the botanical museum at the University of Vienna, in the room of the late Kerner von Marilaun ; G. T. Hastings as assistant in botany at Cornell University ; Dr Franz Hoffmann as private docent for physiology in the University of Heidelberg ; Dr J. Jablonowski, of Berlin, as assistant in anthropology at the Dresden Museum ; Ludwig Katharina as professor of zoology and comparative anatomy in Freiburg University ; George Klebs, of Basle, as professor of botany at Halle University ; Dr Hans Lenk as professor of mineralogy and geology at Erlangen University ; Ludwig Kerschner as professor of histology at the University of Innsbrück ; H. J. Monson and Andrew Linton as senior and junior professors of agriculture to the School of Agriculture at Ghizeh ; W. A. Murrill as assistant in botany at Cornell University ; Lubomir Niederle as professor of archaeology and ethnography in the Bohemian University of Prague ; Dr C. C. O'Hara as professor of geology and mineralogy in the South Dakota School of Mines ; Dr Ph. Pocta as professor of palaeontology at the Ceske Museum, Prague ; Mr Gifford Pinchot, as chief of the Division of Forestry in the U.S. Department of Agriculture, in the room of Dr B. E. Fernow, who becomes the head of the New York School of Forestry ; Dr Aladar Richter as chief of the botanical department of the Hungarian National Museum, and Paul Kuckuck, of Heligoland, to be keeper of botany there ; Dr Ludwig Reh as zoological assistant at the Concilium Bibliographicum, Zürich ; Dr F. J. V. Skiff, of the Field Columbian Museum, as director of mining and mineralogy at the Paris Exposition of 1900 ; C. F. Myers-Ward as lecturer in physiology at the Owens College, Manchester ; Dr A. Weberbauer as private docent for botany in Breslau University ; Dr Zograf, extraordinary professor of zoology, and Dr Mrensbier, extraordinary professor of comparative anatomy in Moscow University.

Lord Peel has been appointed a trustee of the British Museum in the place of the late Mr Spencer Walpole.

Dr C. H. Hitchcock of Dartmouth, U.S., has left for a year's geological exploration in the Hawaiian Islands. His address will be Honolulu.

We regret that we entirely overlooked the fact that the Government made a substantial grant from the Royal Bounty of £150 to Mr Joseph Wright of Belfast

for his long and valuable researches into the palaeontology of the rocks of Ireland. We were quite aware that this had been a possibility for some years past, and hasten to congratulate Mr Wright on his well-deserved distinction.

THE monument to Charcot will be formally unveiled on October 23rd, in the Salpêtrière, Paris.

A LIFE of William Turner of Cambridge, 1507-1568, one of, if not the earliest British zoologist, has been contributed to the *Zoologist* for August, by the Rev. H. N. Macpherson.

A MEMOIR of Fritz Müller, the Brazilian naturalist, is to be undertaken by Dr A. Möller, of Eberswalde. Dr Möller begs the loan of letters or material that will help him in his task.

THE following grants have been made by the Berlin Academy :—2000 marks to Prof. Engler, for East African plants ; 600 marks to Prof. Graebner, for the study of German Heaths ; 500 marks to Dr Loesner, to complete his monograph on the Aquifoliaceae.

MR A. J. HERBERTSON, lecturer on geography in the Heriot-Watt College, Edinburgh, has obtained the degree of Ph.D. *multa cum laude* in geography at the University of Frieburg, in Baden. Dr Herbertson's thesis was on the "Distribution of rainfall over the earth's surface," a subject which he has investigated while compiling the rainfall maps for the physical atlas about to be published by Bartholomew.

THE Hon. John Macgregor has presented a cheque for £500 to the fund for the endowment of a chair of Forestry in the University of Edinburgh. It will be remembered that the Royal Scottish Arboricultural Society has asked the Government for a grant for the establishment of a State Forest near Edinburgh for research in forestry.

THE Swiney Lectures on Geology, under the direction of the Trustees of the British Museum, will be delivered by Dr R. H. Traquair on Mondays, Wednesdays, and Fridays at 5 P.M., beginning Monday, October 3. They will be on the Palaeontology of Great Britain, and will be given in the Lecture Theatre of the South Kensington Museum.

THE New Whale Gallery at the British Museum is the subject of an illustrated article by Mr Lydekker in *Knowledge* for September 1. Owing to the difficulty of position, however, the photograph does not give one a proper idea of the gallery, which is well worthy a visit even from those not specially interested in zoology.

WE learn from *Science* that the Lacoe collection of fossil insects contains the types of about two-thirds of those described from North America. Besides these there are 3500 specimens from the Oeningen Tertiaries, and a large collection from Florrisant, Colorado. The United States National Museum has now perhaps a collection of fossil insects second to none, in any case it has a collection of the first importance.

THE late Professor Victor Lemoine bequeathed his palaeontological collection to the Paris Museum. In order that the collection may be further supplemented Madame Lemoine has handed over the land at Cornay, near Rheims, whence the fossils were obtained, to the same institution.

THE South African Museum has so far advanced as to issue "Annals of the South African Museum," a handsome octavo serial, well illustrated by lithographic plates, and printed and published in London by West, Newman & Co. It will appear at irregular intervals, as matter for publication is available, and will

be devoted to the researches of the museum staff. The first part, which is dated June 1898, contains papers on Scorpions by Purcell, Mutillidae by Péringuey, Reptiles by Sclater, and Hispinae by Péringuey. The Trustees report that the whole work of transferring the collections from the Old Museum to the New Museum Building was accomplished in a month, with practically no damage or loss, at a cost of approximately £90.

The American Museum of Natural History, Central Park, New York, is rapidly progressing with its new buildings. These consist of a corner to the west wing and a lecture hall, the latter of which may be ready for occupancy this year, and the former ready for cases and fittings during 1899. Among the excellent work done by this Museum is the fitting out of expeditions for special objects: thus Dr Carl Lumholtz has returned after four years spent among the tribes in Mexico with a large and valuable collection ; Dr Adolf Bandalier has continued his researches in Bolivia and Peru ; Mr Ernest Volk has been employed for the whole year exploring near Trenton, N.J., for the purpose of careful investigation of the question which has arisen relative to the antiquity of man in the Delaware Valley ; while Mr A. J. Stone begins this year, and will continue till 1900, collecting vertebrate Zoology from Montana to Bering Strait. Two great dinosaurs in a remarkable state of preservation have been secured from Wyoming, and a complete skeleton of the three-toed horse has also been obtained for the collection. The Library grows rapidly, and many scarce works on Zoology have been added to the shelves, while the Duke of Loubat has been a generous donor in the department of Mexican and other ethnology.

The National Herbarium of the United States has received from Dr W. H. Forwood his collection of plants of Western Wyoming, collected in 1881 and 1882. *The Plant World* states that the collection forms the basis of two scarce reports published by the War Department. Many of them are also referred to in Tweedy's Flora of the Yellowstone.

The Report of the Keeper of the Manchester Museum refers to the installation of electric light, which has been rendered possible by the generosity of Mr Reuben Spencer, who contributed £500 to the expense. The Museum is at present in the hands of the painters, and it is to be hoped that the committee will sanction the general whitening of the ceilings asked for by Mr Hoyle, in order that the electric light may have a good start. Prof. Hickson has been doing good work on the plankton of Lake Bassenthwaite, and some of the rarer forms will shortly be placed on exhibition. Miss Nördlinger, the keeper's efficient secretary, receives due eulogies, and we are glad to hear that she has taken entire charge of the library and hope she will be able to open the proper purse-strings for much needed additions. The committee have undertaken the printing of Mr Sherborn's index to the 10th and 12th editions of the "Systema Naturae" of Linnaeus, which should prove of value to zoologists, as these books form the starting-point of zoological nomenclature. A series of lectures will be delivered by Prof. Boyd Dawkins on certain Saturdays and Sundays between October and June, other lectures to be delivered by the staff as usual. Mr Hoyle closes his Report with an eloquent appeal for more funds, and it really does seem singular that Manchester can only afford an expenditure of £2785 a year on its Museum, while Liverpool spends £5700. Manchester must wake up.

A useful part of Mr Hoyle's Report referred to above is his account of the twenty-five museums visited by him while travelling in the United States and Canada in 1897 ; the list, however, is too long to quote here.

The Keswick Museum, which was founded in 1873 in connection with the Keswick Literary and Scientific Society, was removed early this year to Fitz Park,

Keswick, and is now known as the Fitz Park Museum. It owes its origin to the late James Clifton Ward, whose valuable geological work in the lake district is well known. The present curator is Mr James Postlethwaite. The collections are restricted entirely to objects illustrative of the local natural history, and although some of the sections are still far from complete, considerable energy is being displayed to make it exhaustive. A catalogue was issued in 1888, and this, we hope, will soon be followed by a new edition.

Some 3000 members attended the meeting of the British Association at Bristol, September 7-14, under the presidency of Prof. Sir William Crookes. Lectures were delivered in the evenings by Prof. Sollas on "Funafuti, the study of a Coral Island," and by Mr Herbert Jackson on "Phosphorescence," while Prof. Poulton delivered the Working Man's Lecture on "The ways in which animals warn their enemies and signal to their friends." A special biological exhibit was arranged in the Zoological Gardens, consisting of living hybrid trout, specimens of cross-breeding in animals, and hybrid and crossed varieties of flowers, ferns, orchids, and other plants. The First Lord of the Admiralty stationed, by request, four Battleships in Kingroad, Avonmouth, for the edification and protection of the visitors. An excellent series of excursions took place, those most interesting to our readers being Austcliff on Sept. 10 to see the Rhaetic beds, and to Tortworth on Sept. 15 to see the new exposure of Silurian beds recently re-opened by Lord Ducie. This proves to be a thin band of Wenlock bordering the exposure of Upper Llandovery, and is crowded with *Coenites*. A long excursion of five days was taken from Sept. 16-20 to Exeter, Torquay, Dartmouth, Plymouth, and Dartmoor. In a comprehensive pocket handbook which was issued, Prof. Morgan gave a sketch of the geology of the district, Mr J. W. White of the botany, Mr A. E. Hudd of the insects, and Messrs Morgan and Charbonnier of the vertebrata, with the exception of the birds which were dealt with by Mr H. C. Playne.

The Royal Society of Victoria has had a shock not uncommon to societies in the Australian continent, viz., the reduction of its Government grant. We echo the hope of the Council in their last report that "with a return of more prosperous times the vote may be increased so as to enable the Society to publish the papers presented to it." There is a steady growth of the library as indicated by an additional 200 feet of shelving erected during the year.

The Manchester Microscopical Society has issued a satisfactory report for 1897. There is a loss of two members, but that no doubt will be regained next year. The library and the collection of slides are both increasing, and the latter is carefully listed out at the end of the current transactions.

The Edinburgh meeting of the British Medical Association was commemorated by the *Scottish Medical and Surgical Journal* in a special number, issued as volume iii., No. 2, for August, price two shillings. This is well illustrated with photographs of the Presidents, University old and new buildings, M'Ewan Hall, Royal Infirmary, and many Scottish Spas. Among other interesting articles are Medical Institutions in Edinburgh, Medical Student Life in Edinburgh, Edinburgh Medical Clubs, their Songs and Song-Writers, the Edinburgh Royal Infirmary Old Residents' Club, and a general account of Scottish Spas and their mineral waters. A photograph of the Residency table at the Royal Infirmary, covered with names of past residents, will awaken many memories.

The roll of the Field Naturalists' Club of Victoria is 129 members, a slight decrease on that of last year. Its journal, the *Victorian Naturalist*, has commenced its sixteenth year and is edited by Mr F. G. A. Barnard. One of the chief works of the year has been the protection of the albatrosses on Albatross

Island, the result of a deputation to the Premier of Tasmania. They are now safe all the year round for five years. Mr C. French was again elected president.

The Council of the Royal Geological Society of Cornwall in its 84th Report expresses its satisfaction over the new geological survey of the county at the hands of Mr J. B. Hill. Application to the Government was made as the result of the Annual Joint Meeting of the Scientific Societies of Cornwall, at Falmouth, in August 1896, and Mr Hill was told off by the Survey to examine the sections of the south coast last autumn. The Council also reports the complete and detailed examination of the St Erth pliocene, and has under its consideration the preservation of the plans and sections of abandoned mines. Mr Howard Fox has been awarded the Bolitho gold medal, and Mr J. H. Collins has been made an honorary member.

From the Annual Report of the Yorkshire Geological and Polytechnic Society we learn that the roll of members is 164. This is the highest since 1893, and it is satisfactory to learn that all these members are in active association with their Society. The editors have dated their *Proceedings* with the proper year of issue, instead of one year earlier as heretofore. Next year we hope they will improve on this and add the month, for we note that the future bibliographer will not be able to say whether Mr Woodward's paper on the Yorkshire fossil fishes was published in January or December 1898. The *Proceedings* contain a paper on Filey Bay and Brigg by Mr Fox-Strangeways, which is illustrated by eight beautiful photographs by Mr Godfrey Bingley. There are also portraits and obituaries of Thomas Tate and John Stanley Tute.

The Selborne Society in the September number of *Nature Notes* desire to wipe off a printer's debt. The Society is now sufficiently flourishing to show but a small deficit in its balance sheet, but hopes to raise three hundred pounds during the next three years to clear itself of debt.

In June last we called attention to an application for subscriptions to erect a suitable monument to the late Baron Ferdinand von Mueller. This was set on foot by the executors. We now note that a second committee has been formed by Mr W. Wiesbaden, Professor Baldwin Spencer, and others, who are desirous of founding some National Memorial which shall worthily perpetuate his name. Whilst nominally the Government Botanist of Victoria, it is well known that the Baron von Mueller's assistance was sought by and always freely given not only to public bodies but to private individuals in all parts of Australia. Apart from his purely scientific work, upon the value of which it is unnecessary to dwell, Von Mueller devoted himself to the development of the more practical side of various branches of work, such as those connected with Forestry, Agriculture, Horticulture, Pharmacy and, not least, Geographical Exploration. His own explorations in early days, both in Northern Australia as botanist in the expedition under Mr A. C. Gregory, and when, subsequently, he traversed alone the then little known wilds of Gippsland, were of considerable importance, and his deep interest in and the practical assistance which he rendered to the explorations of others are well known. Not only did he spend his whole life in the furtherance of the work in which, from the nature of his position, he was most deeply interested, but he devoted practically the whole of his income to the assistance of those who were engaged in work the object of which was to increase our knowledge of the nature and products of Australasian lands. It is on these grounds, therefore, that the committee hope that sufficient funds will be forthcoming to provide for (1) the erection of some form of statue, and (2) the endowment of a Medal, Prize or Scholarship, to be associated with Von Mueller's name and to be awarded from time to time in recognition of distinguished work in the special branches in which he was most deeply interested, and which

shall be open to workers throughout the Australasian colonies. Subscriptions to the Fund may be sent to the Hon. Treasurer, addressed to the College of Pharmacy, Swanston Street, Melbourne.

THE Report of the Botanic Gardens and Domains of New South Wales for 1897, by Mr J. H. Maiden, has recently appeared, and contains full accounts of the Botanic Gardens, Government Domains, Garden Palace Grounds, Centennial Park, State Nursery at Campbelltown, &c. Mr Hugh Dixson has placed his collection of Australasian orchids at the disposal of the Botanic Gardens, and suitable accommodation is to be speedily provided for their reception. Parliament has also voted a sum of money for the erection of a building to house the Herbarium ; the Library shows a steady progress. Altogether a very favourable and hopeful report, and the first of a new series, which is to be continued annually. The last report appeared in 1878.

WE learn from the *Echo* that 100 tuns of beer and 18,000 cups of coffee were consumed at the Berlin Zoological Gardens on Whitsun Day. We are not responsible for the statement, but, if true, it shows that zoology as an interest is not likely to die out in Berlin.

"NATURE" for August 25 has an interesting article on "The Marine Fauna in Lake Tanganyika and the advisability of further exploration in the great African lakes," by J. E. S. Moore. Mr Moore prints a list of empty shells and fishes previously known and also a list of the entire mollusca and fishes obtained during his expedition.

THE Swiss Society Rambertia has laid out an Alpine Garden at Montreux, at an elevation of 6000 feet, where the characteristic trees and flowers of the country are to be cultivated.

AT the moment of going to press we learn that Dr Florentino Ameghino has made a remarkable discovery. Details of a nocturnal quadruped have been brought to him from time to time by Indians, and a few years ago the late Ramon Lista actually saw and shot at a mysterious creature in the interior of Santa Cruz. Apparently bullet-proof, it disappeared into the brushwood, and all search for it proved futile. Lista described the creature as a pangolin, without scales, and covered with reddish hair. Despite the fact that Lista was known to be a good observer, Dr Ameghino could not help feeling that he was deceived. Lista, however, has now been proved correct, for Ameghino received recently from South Patagonia some fresh bony ossicles and a partially destroyed skin. The ossicles were comparable to those of *Mylodon*, but smaller, and they were embedded in the skin, like "paving stones in a street." The skin itself is two cm. thick, and of such toughness that it could only be cut with a hatchet. The surface of the skin itself shows an epidermis, not scaly at all, but covered with coarse hair, four to five cm. in length. and of a reddish grey shade. This Ameghino considers was the animal described by Lista, and as that naturalist has unfortunately lost his life while exploring Pilcomayo, and was the only civilised man who had seen it in the flesh, he names it *Neomylodon listai.* The importance of the discovery need not be emphasised here.

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 29 and 30 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

DEC 10 1898

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 81—Vol. XIII—NOVEMBER 1898

NOTES AND COMMENTS

Found—an Editor

Our readers will be glad to learn that *Natural Science* will continue to appear as heretofore during 1899 and, we hope, for many years to come. Arrangements have been made for its transfer to an Editor in whom we have every confidence, and who has undertaken to continue the journal on the lines with which our readers are familiar. Particulars as to the future editorial and publishing offices will be given in our December number.

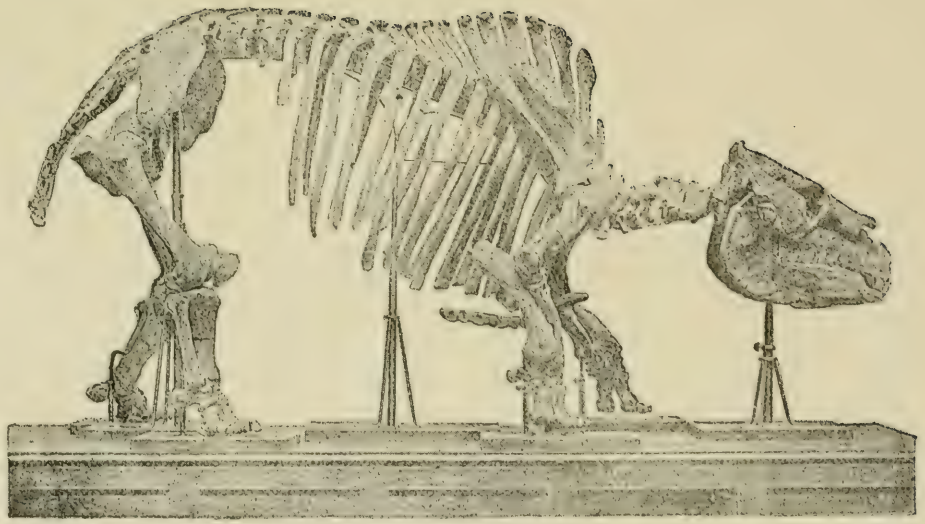
Distribution of the Oceans and Continents

In discussing the theories of the distribution of the Oceans and Continents before the British Association, Dr J. W. Gregory remarked that the "main object of geomorphology is to explain the existing distribution of land and water on the globe. A remarkable series of coincidences in the form and arrangement of the land masses suggests that the distribution has been determined by some general principle and not by local accidents. The three most striking features that require explanation are the antipodal position of oceans and continents, the triangular shape of the geographical units, and the excess of water in the southern hemisphere. Attempts to explain this arrangement have been made deductively from general physical considerations, as by Elie de Beaumont, Lowthian Green, and G. H. Darwin; and directly from the evidence of stratigraphical geology as by Suess, Lapworth, and Michel-Levy. Thus Elie de Beaumont regarded the form of the continents as determined by the mountain chains, which he correlated into a regular geometrical network; while Lapworth regarded the distribution of land and water as due to a series of great earth-folds, the arches forming the continents, and the troughs forming the ocean basins. Suess has treated the

subject synthetically; he has shown that the structure of the world can be explained by subsidences in the crust, when subterranean support is removed by the shrinkage of the internal nucleus, and by the movements of elevation which produce the chains of fold-mountains. Suess's view explains the structure of the continents and ocean basins, but not their arrangement. To settle this problem fuller knowledge is needed as to the distribution of land and water in past times. Neumayr's attempt to settle this question for the Jurassic was premature, and his conclusions are untenable. We are thus still dependent upon the deductive systems for suggestions as to the most profitable lines of research. Elie de Beaumont's famous scheme attached undue importance to linear symmetry and was too artificial. It led, however, to the tetrahedral theory of Lowthian Green, which regards the world, not as shaped like a simple tetrahedron, but as a spheroid slightly flattened on four faces. Such flattenings occur on hollow, spherical shells, when they are deformed by uniformly distributed external pressure. The oceans would occupy the four depressions thus produced, while the land masses occur at the angles and along the edges. The existing geographical arrangement is in general agreement with this scheme; for as the tetrahedron is hemihedral the assumption that the lithosphere is tetrahedral explains the antipodal position of land and water, the excess of water in the southern hemisphere, and the southward tapering of the land masses. The main lines of the existing system of fold-mountains have a general agreement with the arrangement of the edges of a tetrahedron. Some striking deviations occur, but are explicable by the variations in the composition of the lithosphere, and the existence of impassive blocks of old strata which have moulded the latter movements. The lines of the old fold-mountains of the Hercynian system may have been tetrahedrally arranged, but with the axes occupying different positions from those of the great Cainozoic mountain system. So far, however, there is no completely satisfactory theory of geomorphology, for which we must wait for further information as to the distribution of land and water in successive epochs of the world's history. For the historical method promises more reliable results than the deductive method."

Toxodon

The important serial publications of the Museum of La Plata, Argentina, contain some of the most valuable contributions to natural science which have been made during the last decade. We have had frequent occasion to refer to them, and to the vast store of unique specimens which the energy and genius of Dr F. P. Moreno have accumulated in the comparatively new capital of the State of Buenos Aires. The *Revista* and *Anales*, however, contain only



SKELETON OF *Toxodon*,

AS MOUNTED IN THE LA PLATA MUSEUM

general memoirs and papers, without any detailed statistics of the Museum collections. The Director has thus wisely decided to begin a third set of publications, namely, a series of Catalogues recording the specimens in the Museum under their register-numbers, with brief descriptions and illustrations, somewhat on the plan of the Catalogues of the British Museum. The first of these publications, just issued, relates to the Department of Palaeontology, and is entitled "Catálogo de los Mamiferos Fósiles conservados en el Museo de la Plata." Only the first section is before us, namely that comprising the type-genus of the ungulate order, Toxodontia, by Dr Santiago Roth, curator of the fossils. No less than 128 pages, with 81 text-figures and 8 plates, are devoted to this characteristic genus of extinct South American hoofed animals. The well-known figure of the nearly complete skeleton of *Toxodon* at La Plata, which we reproduce herewith, has now found its way into most recent text-books. The student of mammals has thus known for some years what to expect from a detailed account of the specimens of *Toxodon* in the La Plata Museum; and now for the first time he is furnished with tolerably adequate descriptions. The characters of the various parts of the skeleton are first systematically described; and then the species are successively diagnosed, while a numbered list of specimens is placed beneath each. Although numerous new facts are recorded, it would still be premature to say more concerning the affinities of *Toxodon* than has already been said by previous observers. We now want to know more of *Nesodon* and its ancestors, which are found in Patagonia, before the relationships of this strange group of ungulates can be further discussed.

Fossil Ostrich in China

About 1857, a remarkable fossil egg was discovered at Malinowka, Government of Cherson, S. Russia, which though now destroyed and lost, was seen by Prof. A. Brandt of Charkow and described by him in the Bulletin of the St Petersburg Academy in 1873. Nathusius examined some of the fragments microscopically, and declared that they indicated a very close relationship with the common ostrich. The egg as a whole, however, had a cubic contents of upwards of 2075 c.cm., while the largest known egg of the ostrich has only two-thirds this capacity. The microscopical structure being very characteristic of the group he referred the egg to a new genus and species *Struthiolithus chersonensis*. No bones of the bird that left behind the egg are known, but ostrich remains have been described from the Pliocene of the Sivalik Hills and the lower Pliocene of Samos. The fragments of the Cherson egg are still preserved in St. Petersburg Museum. Considerable interest is now attached therefore to a paper by Mr C. R. Eastman, which

forms number 7 of the thirty-second volume of the *Bulletin of the Museum of Comparative Zoology at Harvard College*, in which is described and figured an entire and perfect specimen of an ostrich egg which was found a few years ago by a Chinese farmer at Yao Kuan Chuang, district of Hsi Ning, about fifty miles south south-west from Kalgan. The find consisted of two specimens, one of which was broken, and is perfectly well authenticated by the Rev. Wm. P. Sprague, who visited the spot in company with the man who found them and secured the unbroken specimen, which is now in the Harvard Museum. According to Mr Sprague's account, corroborated by references to Richthofen's China, the deposit from which the egg came was Loess. The egg itself presents almost exactly the same appearance as the Russian egg, of which a plaster cast is preserved, and in the opinion of Mr Eastman it may be considered at present to belong to the same bird. The cubic contents of the Chinese egg is 1896·90 c.cm. The occurrence of fossil ostrich remains in the Loess of such widely separated regions as Northern China and Russia has a direct bearing upon the distribution of Struthious birds, and gives rise to some important inferences by Mr Eastman, regarding the past history of Ratite birds in general.

The Notes of Birds

Many a wanderer in the country has wished that he could identify the various birds that he hears singing on the hedges or calling in the fields. Those who live in the country often know the call, but can only identify the bird by its local name. Mr Charles Louis Hett of Brigg has produced a small octavo volume, handy for the pocket, which is to be obtained for half-a-crown of Messrs Jackson, Market Place, Brigg, which gives these notes and calls arranged in alphabetical order, most of which give a fair idea of the various sounds produced. Further than this, Mr Hett has also given a list of the popular local and old-fashioned names of British birds, under each of which the notes are repeated, and closes his little volume with a list of the scientific names of all birds accepted as British by the British Ornithologists' Union in 1883. Equipped therefore with this volume, the bird lover may identify, with a certain approach to accuracy, many of the birds met with in his rambles, and what is of greater importance, may, now he has a basis to go upon, try and record more accurately the delusive and fugitive calls of many of the species.

Life Conditions of the Oyster

The following conclusions of the Committee appointed by the British Association to report on the elucidation of the life conditions of the

oyster under normal and abnormal environment, including the effect of sewage matters and pathogenic organisms, drawn up by Professor Herdman, Professor Boyce, and Dr Kohn, are, we think, of sufficient interest to the public to repeat here in full.

1. There are several distinct kinds of greenness in oysters. Some of these, such as the green Marennes oysters and those of some rivers on the Essex coast, are healthy; while others, such as some Falmouth oysters containing copper and some American oysters re-bedded on our coast and which have the pale-green leucocytosis we described in the last report, are not in a healthy state.

2. Some forms of greenness (*e.g.*, the leucocytosis) are certainly associated with the presence of a greatly-increased amount of copper in the oyster, while other forms of greenness (*e.g.*, the Marennes) have no connection with copper, but depend upon the presence of a special pigment Marennin, which may contain a certain amount of iron.

3. We see no reason to think that the iron in the latter case is taken in through the surface epithelium of the gills and palps; but regard it, like the rest of the iron in the body, as a product of ordinary digestion and absorption in the alimentary canal and liver.

4. We do not find that there is any excessive amount of iron in the green Marennes oyster compared with the colourless oyster; nor do the green parts (gills, palps, &c.) of the Marennes oyster contain either absolutely or relatively to the colourless parts (mantle, &c.) more iron than colourless oysters. We therefore conclude that there is no connection between the green colour of the Huitres de Marennes and the iron they may contain.

5. On the other hand, we do find by quantitative analysis that there is more copper in the green American oyster than in the colourless one; and more proportionately in the greener parts than in those that are less green. We therefore conclude that their green colour is due to copper. We also find a greater quantity of iron in these green American oysters than in the colourless; but this excess is, proportionately, considerably less than that of the copper.

6. In the Falmouth oysters containing an excessive amount of copper, we find that much of the copper is certainly mechanically attached to the surface of the body, and is in a form insoluble in water, probably as a basic carbonate. In addition to this, however, the Falmouth oyster may contain a much larger amount of copper in its tissues than does the normal colourless oyster. In these Falmouth oysters the cause of the green colour may be the same as in the green American oysters.

7. The Colon group of bacilli is frequently found in shellfish, as sold in towns, and especially in the oyster; but we have no evidence that it occurs in Mollusca living in pure sea-water. The natural inference that the presence of the Colon bacillus invariably indicates sewage contamination must, however, not be considered established without further investigation.

8. The Colon group may be separated into two divisions—(1) those giving the typical reactions of the Colon bacillus, and (2) those giving corresponding negative reactions, and so approaching the typhoid type; but in no case was an organism giving all the reactions of the *B. typhosus* isolated. It ought to be remembered, however, that our samples of oysters, although of various kinds and from different sources, were in no case, so far as we are aware, derived from a bed known to be contaminated or suspected of typhoid.

9. Consequently, as the result of our investigations, and the consideration of much evidence, both from the oyster-growers' and the public health officers' point of view, we beg to recommend :—

(*a*) That the necessary steps should be taken to induce the oyster trade to remove any possible suspicion of sewage contamination from the beds and layings from which oysters are supplied to the market. This could obviously be effected in one of two ways, either (1) by restrictive legislation and the licensing of beds only after due inspection by the officials of a Government department, or (2) by the formation of an association amongst the oyster-growers and dealers themselves, which should provide for the due periodic examination of the grounds, stores and stock, by independent properly-qualified inspectors. Scientific assistance and advice given by such independent inspectors would go far to improve the condition of the oyster beds and layings, to reassure the public, and to elevate the oyster industry to the important position which it should occupy.

(*b*) Oysters imported from abroad (Holland, France, or America) should be consigned to a member of the "Oyster Association," who should be compelled by the regulations to have his foreign oysters as carefully inspected and certificated as those from his home layings. A large proportion of the imported oysters are, however, deposited in our waters for such a period before going to market that the fact of their having originally come from abroad may be ignored. If this period of quarantine were imposed upon all foreign oysters, a great part of the difficulty as to inspection and certification would be removed.

(*c*) The grounds from which mussels, cockles and periwinkles are gathered should be periodically examined by scientific inspectors in the same manner as the oyster beds. The duty of providing for

this inspection might well, we should suggest, be assumed by the various Sea Fisheries Committees around the coast.

The Method of Feeding of *Helix hortensis*

Mr E. Rathay publishes an interesting article on the method of feeding of *Helix hortensis*, in the third part of Vol. viii. of the *Zeitschrift für Planzenkrankheiten.*

The author had noticed on the smooth bark of ash-trees certain undulating patterns, in the immediate neighbourhood of which, or at their extremities, were individuals of *Helix hortensis.* Mr Rathay therefore felt that the patterns must be accounted for by the snail's method of feeding.

To make certain of the fact, he took some bits of ash-bark on which no patterns had been traced, set them in an upright position so as to keep them fresh in the water, put a *Helix* on each, and covered them over with a glass bell. The very next day, these bits of bark showed traces of undulating patterns.

In consequence of this experiment, the author's attention was drawn to other smooth-barked trees, and he recognised the same patterns on *Salix caprea L., S. amygdalina L., Alnus incana C.*, &c.

These traces were noticed on the trunks to the height of 7 to 9 metres, and the snail that was observed to be at work, produced them by slowly advancing his body and swaying his head alternatively to right and left.

One might suppose that the gasteropod fed himself thus by gnawing the bark of the tree, but it is nothing of the kind; he attacks the spots where the bark is powdered with a small alga, *Pleurococcus vulgaris*, Menegh., and scarcely touches the outer skin of the bark.

In fact, in the excrement of *Helix hortensis* are found the cells of *Pleurococcus* almost intact.

In accordance with the experiments detailed in the note and after the employment of various appropriate reagents, it is recognised, not only by microscopic examination, but by chemical experiment, that the cells of *Pleurococcus* have been evacuated intact with their chlorophyl.

The author's conclusions are as follows :—

1. It is only on smooth barks sufficiently coated with alga that the undulating patterns can be detected.

2. It can easily be observed, especially on the older trees, that *Helix hortensis* does not eat the outer skin of the bark and scarcely touches it.

3. The excrement of the same gasteropod, taken a great height up the tree, is chiefly composed of cells of *Pleurococcus* with very few fragments of peridermis. The extraordinary thing about

this is that these cells are evacuated apparently intact, not only with their chlorophyl, but with the other substances that they contain.

4. It has been noticed that *Helix hortensis* produces the same patterns on the lattice work of a wooden fence covered with *Pleurococcus vulgaris*. A figure of the pattern referred to will be found reproduced in *La Feuille des jeunes Naturalistes*, September, from which journal we take this note.

Abnormal Shells of Planorbis

The abnormalities of our fresh-water snails will be no new fact to bring before the notice of our readers, but attention may well be directed to a paper by Mr A. G. Stubbs on abnormal specimens of *Planorbis spirorbis* from Tenby. The paper was read before the Conchological Society, and is published in the October number of their *Journal*. A good plate is provided, and the shells are seen to be contorted in every direction, but mainly into that of a spiral, some of these so much drawn out as to be nine or ten times the height of the normal shell. Mr Stubbs accepts Mr J. W. Taylor's explanation as to the cause of this curious distortion :—" that when the water [in this ditch] is nearly dried up, the efforts of the creatures in forcing their way through the thick mud in which they are sometimes left partially embedded, to again reach the water, may easily cause an alteration in the direction of a new shell growth, if at the time in course of formation."

A Remarkable Marine Organism

Among a number of sponges from Rámésvaran Island, Gulf of Manaar, sent to Dr Arthur Dendy for identification for the Madras Museum, were some fifteen specimens of cushion-shaped masses of a brown colour, from 13 mm. to 36 mm. in diameter, attached to rock fragments. These masses are compact and tough in texture, after preservation in spirit, like indiarubber, and there is a deal of sand in the deeper layers. When cut in half vertically they show strongly-marked, concentric lamellae, the effect of alternating bands of flocculent (opaque) and transparent layers. The opaque layers are connected together by a coarse network of radially ramifying strands. In the transparent layers are seen, after careful examination, innumerable exceedingly slender unbranched threads, which prove to be the cellulose sheaths of chains of short, rod-like bacteria. Dr Dendy thinks that there are two possible views as to the nature of *Pontobolbos*, as he calls this remarkable structure, and these are (1) that the organism is entirely bacterial in origin, the

opaque layers and network being due to the formation of slime or jelly by the bacteria themselves, or (2) that it is due to symbiosis between the bacteria and some gigantic rhizopod, the protoplasm of which is seen in the flocculent layers and network. After discussing these two views, Dr Dendy calls attention to the marvellously close resemblance of *Pontobolbos* to the enigmatical fossils Stromatoporidae. The paper appeared in the Linnean Society's Journal, volume twenty-six.

'Bugs' as Food

Mr G. W. Kirkaldy, who has been devoting his attention for some time to the systematic description of the Rhynchota, has written a short note to the *Entomologists' Monthly Magazine* (p. 173) on arrangements which have been made for the importation into this country both of imagines and ova of *Notonecta* and *Corixa* in large quantities for the food of insectivorous birds, game, fish, and others with peculiar tastes. It has long been known that the natives of parts of Mexico eat the perfect insects with relish, and that the sale of cakes made of the ova is fairly extensive. The species to be imported, according to Mr Kirkaldy, are *Notonecta americana*, Fabr., and *Corixa mercenaria*, Say. The ova are called by the Mexicans 'Axayacatl' or 'waterface,' and are made into cakes with the addition of meal. These are eaten *au naturel* or with green chilies. If cooked without meal they are called 'ahuanhtli' or 'water wheat,' have the appearance of fish roe, have a delicate flavour, and are not disdained at fashionable tables. Virlet d'Aoust indeed compared them to caviare. Mr Kirkaldy, however, cannot speak highly of them as a relish, his were stale and tasted of sulphuretted hydrogen and decayed animal substances. The perfect insects, moreover, had a distinct 'buggy' flavour. Still one can educate one's palate, and there are some who revel in the pope's nose of a goose despite its taste of cockroach. The *C. mercenaria* are imported by the ton, and each ton is imputed to contain 250 millions of insects. We will not dine with Mr Kirkaldy.

The Relationships of Butterflies

Readers of *Natural Science* will recall Mr A. Radcliffe Grote's papers at the beginning of the present year (vol. xii. pp. 15-26, 87-99) on the classification of butterflies according to the wing-neuration. His main contention was the separation of the Papilionidae from all the other butterflies on account of the presence of a short vein (ix.) next to the inner margin of the forewing, this vein being absent in all the other families. In a recent paper entitled "Specialisations of the Lepidopterous Wing; the Pieri-Nymphalidae," in the *Proc. Amer. Phil. Soc.* (vol. xxxvii. pp. 17-

44, pls. i.-iii.), Mr Grote considers in detail the neuration in certain genera of the "Whites" and the "Brush-footed butterflies," which he believes to be rather closely related to each other.

Meanwhile Dr K. Jordan has published a study of butterfly-feelers ("Contributions to the Morphology of the Lepidoptera; the Antennae of Butterflies," *Nov. Zoolog.*, vol. v. pp. 374-415, pls. xiv., xv.) which has led him in many respects to conclusions at variance with those of Mr Grote. From the amount of scaling on the feelers, and the arrangement of grooves with sensations on the ventral surface of their segments, he associates the Nymphalidae and Papilionidae together as the most highly specialised butterflies. In the Nymphalidae there are two ventral grooves on each segment, in the Papilionidae either two or one; the latter condition occurring in the Parnassinae, but their single groove apparently representing one of the lateral grooves in the Papilios. Among the other families, there are either no ventral grooves (Hesperiidae, Lycaenidae), or one (Erycinidae, some Pieridae), or three (other Pieridae).

Dr Jordan's valuable research will be welcomed by all students of the Lepidoptera, and no one can doubt that such characters as he indicates must be taken into account in the discussion of affinities. At the same time, a comparison of his results with those of Mr Grote raises the question whether it is advisable to erect a phylogenetic classification on facts relating only to one set of organs. We are constantly receiving fresh light as new structures are studied, and several modifications of current arrangements seem to be supported by converging lines of evidence. For instance pupal structure (Chapman), wing neuration (Grote), and antennal characters (Jordan) all combine to indicate that the Hesperiidae and Lycaenidae must be regarded as the most primitive families. On the other hand, Mr Grote's association of the Pieridae with the Nymphalidae and their allies, while supported by the pupal characters elucidated by Dr Chapman, is, as we have seen, contradicted by the feelers as interpreted by Dr Jordan.

It is of interest to note that Mr Grote and Dr Jordan, in these two papers, agree in restoring to the Pieridae the abnormal West African insect *Pseudopontia* (or *Gonophlebia*) *paradoxa*, which Dr Butler and others have been inclined to regard as a moth. Both its wing-neuration and antennal structure prove it to be an abnormal pierid butterfly.

An Entomological Controversy

In the same volume of the *Novitates Zoologicae* (pp. 435-455) Dr Jordan replies to some aspersions cast on the work of himself and Mr W. Rothschild, by the late Professor Eimer in his recently published "Orthogenesis der Schmetterlinge." Dr Jordan is apparently

in agreement with several of Eimer's conclusions—the inheritance of acquired characters and the small influence of natural selection in the origin of species. His trenchant criticisms of the statements and reasonings by which Eimer supported his conclusions are therefore all the more weighty.

The Origin of Diatomaceous Earths in New Jersey

The lacustrine sedimentary deposits of Weequahick Lake, Newark, New Jersey, have been long considered as fresh water deposits of diatomaceæ. Professor Arthur M. Edwards has been recently studying these deposits as represented in the valley of the river Passaic, and in the clay there, which is three feet thick, has found a mixture of marine and fresh water diatoms. He also finds numerous kettle-holes and deposits of a peaty matter all of which contain the diatomaceous earth. From this he concludes "that the whole country in North America, and most likely in Europe also, was covered by a fresh-water sea, derived from the melting ice at the period when icebergs made their appearance, and that the temperature of this sea was 0° C. (32° F.), because that is the temperature most congenial to the bacillarias; and the diatomaceous clays described above were laid down as fresh-water deposits from this sea during the iceberg period." The paper forms pp. 103-107 of a Society which is apparently ashamed of its name, for that nowhere appears on the excerpt.

The Persistence of Specific Forms

In the above paper is a remark that all the forms noted are of the same kind as are found in various parts of the world; while in a brief note by the same author, and published so long ago as March 1897 in the *American Monthly Microscopical Journal*, we read with reference to some "Tuscarora" soundings: "The same forms are to be found in the Neocene of California whenever it has been examined, from Crescent City in Del Norte county on the north to a spot about forty miles south of the southern limit in Southern California, that is to say into Mexico. They are the same in the infusorial earth of the Atlantic Coast of North America, and likewise in South America when it has been detected at Payta and Mejillones in Peru. In North America it is known as Miocene territory and is seen at Atlantic City in New Jersey, at Richmond in Virginia, at various points in Maryland, as at Nottingham, and at Tampa Bay in Florida. It is likewise known at Oran in Africa, at Moron in Spain, at Mors in Denmark, at Catanisetta in Sicily, at Simbirsk in Russia, and at Senz Peter in Hungary. Besides, it is known at Netanai in Japan and Oamaru in New Zealand."

"And what does this bring us to? We have to compare the

forms of Bacillaria, Rhizopoda and Foraminifera of these different localities and we find them essentially the same in all. We have also to compare the forms of Bacillaria, Rhizopoda and Foraminifera of the soundings in the Pacific and Atlantic oceans and we find them the same. Can we not say that the strata are the same in composition chemically and the same in organic forms?"

"I think they are. And can we separate the Neocene from the recent soundings in any respect? I do not think so. . . . We cannot distinguish Neocene Bacillaria, Rhizopoda or Foraminifera from recent which are living now. Although the strata in New Zealand have been placed in the Cretaceous, and at Simbirsk in the lower Eocene, we must expect to see them bearing like forms to the recent, and which live more on the bottom of the ocean and are in every inlet along the coast." Much of this has been said before, but it will well bear repetition.

A Tidal Crannog at Dumbarton

An undoubted crannog of a remarkable type was found recently by the well-known archaeologist, Mr W. A. Donnelly. It is the first of such structures found in tidal waters. The discovery has been inspected by Drs Anderson and Munro, and the latter after making a thorough investigation of the site, declared that "it was the most curious, puzzling, and interesting find of the kind he had met with in all his long experience, and, so far as he knew, unique." Mr H. J. Dukinfield Astley, who communicated this find to the *Athenaeum* (Sept. 10), says that Mr Donnelly, with the help of the Helensburgh Naturalist and Antiquarian Society, has thoroughly investigated the spot with a rich reward. The crannog is 1000 yards east of the Castle Rock of Dumbarton, and about 2000 yards from Dunglass Castle, below high-water mark, and about 50 yards from the river at low tide; when the tide is in it is submerged from 3-12 feet. The approach is from the north. The circuit of the crannog is 184 feet. The piles in the outer circle are of oak, which below the mud surface is still quite fresh; the transverse beams and pavements inside are of wood of the consistency of cheese—these are of willow, alder, and oak; the smaller branches are of fir, birch, and hazel, with bracken, moss, and chips. The stones in the outer circle and along the causeway leading to the dwelling-place seem to be placed in a methodical order, most of the boulders being about a lift for a man. The refuse-mound extends for about 12 feet outside for the greater part of the circuit, and in this the flint and bone implements have been chiefly discovered, while near the crannog itself a canoe, 37 feet long and 40 inches beam, dry ends of an oak tree, was also found.

As regards its construction—of stones, wooden piles and pave-

ments—shape, and the finding of canoe alongside, this crannog differs in no way from other well-known ones in Ireland and elsewhere; but in two respects it is absolutely unique: (1) as was stated above, in being situated on the shores of a tidal river; and (2) in the fact that, so far at any rate, none but implements of flint and bone have been discovered. This would throw its occupation back at least to the Neolithic period, whereas crannogs are usually associated with the Bronze Age, *e.g.*, the British lake village at Glastonbury yielded beautiful specimens of bronze fibulae and other articles. Details as to further finds will, therefore, be eagerly awaited by archaeologists.

Australian Initiation Ceremonies

Light is gradually being let into the remarkable ceremonies of initiation that the young Australian has to pass through before he is admitted to the secrets of the tribe and regarded as a full member of it. Much has been published by the Horn Expedition, and by a recent Government publication, but Professor Baldwin Spencer and Mr F. J. Gillen have now given us the full details of these interesting ceremonies as performed by the Arunta tribe of Central Australia. Mr Gillen is a Sub-Protector of the aborigines, and so has special opportunities of observing, and much of what was glossed over by the earlier observers is now carefully related and explained. Excepting, perhaps, one tribe, the Wótjo-balluk of the Wimmera district, Victoria, every Australian native has to undergo these ceremonies. In the case of the tribes inhabiting the east and south-eastern coastal districts of the continent, the ceremonies appear to be entirely distinct from those of the tribes of the central area, amongst whom they are very elaborate and spread over a long series of years, the first taking place at about the age of ten or twelve, whilst the final and most impressive one is not passed through until the black fellow has reached the age of at least twenty-five or even thirty. The ceremonies described in the *Transactions of the Royal Society of Victoria*, vol. x. part 2, are four in number, and are (1) the Enchīchīchĭka and Alkīrakiwūma, or painting and throwing the boy up; (2) Lartna or circumcision; (3) Ariltha or subincision; and (4) Engwurra or fire ceremony. One of the most noticeable features of the Arunta ceremonies is the absence of the knocking out of the teeth, but no doubt to-day much of the ceremony in various tribes has lost its old significance, and degenerated or developed along different lines as the tribes separated from their original common centre.

The Australian aborigines also form the subject of a paper by Mr Oliphant Smeaton this month in the *Westminster Review*, who deals with their curious legends.

Natural Gas in Sussex

So long ago as 1875 Mr Henry Willett noticed the discovery of an inflammable natural gas while conducting the Netherfield Boring. In 1895 another discovery was made while boring for water at the new Heathfield Hotel, in the parish of Waldron, East Sussex. Here, at a depth of 228 feet, the foreman noticed that the water in the bore was "boiling," and on applying a candle the gas caught fire and burnt "to about the height of a man." The third discovery was made in August 1896, formed the subject of a paper in our August number, and is now fully described by Mr Charles Dawson and Dr J. T. Hewitt in the Quarterly Journal of the Geological Society for August 1898. About 100 yards from the hotel, in a cutting, the London and Brighton Railway Company, desiring better water supply for their engine tank, put down a 6-inch bore. Gas was noticed for some time, but when the bore had reached 312 feet from the level of the permanent way, the rush was so pronounced that on a match being applied a flame shot up, which was extinguished with difficulty by damp cloths. The gas continued to increase in volume, but as the bore failed to supply the necessary water, it was abandoned at 377 feet. The tubes were then withdrawn, with the exception of the last, to which an iron cap was screwed with an $\frac{1}{2}$-inch bend and stop-cock. A continuous escape for eighteen months has occurred with a pressure of 15 lbs. to a square inch in March and one of 20 lbs. on June 11 this year. Analyses were made by Mr S. H. Woodhead, which gave :—

Oxygen	18
Higher hydrocarbons . .	5·5
Carbon monoxide . .	4·0
Marsh gas	72·5
	100·0

and by Mr Hewitt, which gave :—

Methane (CH_4) . . .	91·9
Hydrogen (H_2) . . .	7·2
Nitrogen (N_2) . . .	0·9
	100·0

The lowest part of the bore seemed to be in the Purbeck strata, which are known to contain a little bituminous matter. But it seems more likely that the gas comes from the underlying Kimmeridge clay, which was richer in petroleum the lower it was penetrated by the Sub-Wealden Boring in 1875.

The Mount Rainbow Gold-Field, Queensland

The basalt capping of the flat-topped and steep-sloped hills of the Mount Rainbow gold-fields in Queensland rest upon a sediment of wash of 2 or 3 feet of rounded and subangular pebbles and boulders of granite, quartzite, and other rocks of the Gympic formation, cemented in a grit of quartz, felspar, hornblende, and mica grains, overlaid by a white tenaceous, horizontally bedded, clayey sand. This latter deposit is often 10-15 feet in thickness, and rests on a horizontal floor of granite. This wash averages gold to the amount of 1 oz. 11 dwts. 18 grs. per ton, and the cost for crushing is 12s. 6d. per ton, as against £1 per quartz. Much useless material has to be crushed, owing to the hardness of the cement. The gold occurs in rounded or flattened water worn grains, and is all obtained from the lowest 2 or 3 inches of wash, and the uppermost 2 or 3 inches of decomposed granite floor. A full account of the geology will be found in No. 126 of the Geological Survey publications.

Virchow's Lecture

As the *Saturday Review* reminds us, the selection of Professor Virchow as this year's Huxley lecturer was a quaint method of doing honour to Huxley's memory. The lecture itself was a brilliant statement of the growth of the cellular views of pathology and their influence on medical work,—an eminently suitable subject with which to associate Huxley's name. Virchow sketched the growth of theories regarding vitalism and the gradual development of the cellular theory. He insisted in its corollary that the organism is not an individual but a social mechanism. He referred to the application of the cellular theory to pathology due to his own work, which was an indirect outcome of the biological principle *omnis cellula e cellula.* This principle also explains heredity, while it overthrows some of the most elaborately constructed theories as to the hereditary nature of some diseases. Modern theories of malaria, antiseptic surgery and artificial immunization against diseases are also consequences of the theory of cellular pathology. In the early part of the lecture Virchow paid a warm tribute to Huxley, admiration for whom he said "is deeply rooted within me." But later on there came an unnecessary reminder of former controversy by the remark that "Huxley had no hesitation in filling the gaps which Darwin had left in his argument," and by a reservation that "whatever opinion one may hold as to the origin of mankind," so that Virchow now, as in 1895, is still opposed to the application of evolution to man.

551.77
593.12
563.12

I

The probable depths of the Gault Sea as indicated by its Rhizopodal Fauna

WHEN we consider the evidences of variability in the forms of foraminifera, and their power of adaptation to limited amounts of change in their environment, it may seem futile to attempt to attach much value to these organisms, as indices of the bathymetrical and other conditions of the deposit in which they are found. Whilst recognising this power of adaptation, however, we must not lose sight of the fact that marked changes are visible in the aspect of assemblages of foraminifera. For example, when we pass from material which has been laid down in clear and deep water in proximity to limestone cliffs, to other, and it may be, adjacent, and contemporary deposits, subjected to inroads of muddy and decomposing organic material, we shall probably find that, whereas in the former case the species are well-developed and thick-shelled, in the latter case the foraminifera will be thin-shelled and starved, or perhaps with tests formed, of necessity, of the minute sand-grains of the deposit upon which they lived. A case in point is afforded by the limestones and black clays of the Rhaetic of Wedmore Hill in Somerset (1).

The copious records of foraminifera from known depths, made by the Challenger and many other important dredging and sounding cruises, supply us with a tangible basis for the comparison of types of foraminifera which are found in both recent and fossil accumulations.

In consequence of the nature of the sea bottom, its temperature and depth being to some extent interdependent, we may gather many interesting facts by a due consideration of all these points.

In the present instance those species from each zone of the Gault (2) have been taken, which occur also in recent deposits, where their known depths have been accurately recorded. These depths have been carefully averaged for each species selected, and the total mean depth for all the species in each distinct zone of the Gault has been taken as the probable depth of its sea bottom. In cases where there is a preponderance of common and well-developed forms, the evidence of such is considered, to the subordination of occasional examples, which may have been introduced into the

deposit by the action of currents. In obtaining the data from the recent species attention has been especially paid to evidence of depth at which the particular species occurs most frequently, and where it attains its best development.

The material on which these calculations are based cannot afford a complete comparison on account of the number of Gault forms which are quite unknown in recent deposits, but this notwithstanding, an approximation to the truth may perhaps be obtained.

The noteworthy and important groups of foraminifera found in the Gault, and which are not exactly represented in our recent faunas are—

(1) The strongly costate Nodosariae;

(2) The complanate and limbate Frondicularіae;

(3) The sulcated and limbate Vaginulinae;

(4) The remarkably developed and attached Ramulinae, the allied genus *Vitriwebbina*;

(5) And the limbate, reticulate, and spinose Pulvinulinae.

The comparatively large size and redundant growth of these forms indicates favourable conditions for development, in which a high bottom temperature and a sufficiency of calcareous material dissolved in the water would form important factors. Another possibly important condition was the accumulation of marine shells over which this part of the Benthos of the Gault Sea was able to wander, and amongst which it could shelter. In the case of the Ramulinae and Vitriwebbinae these curious recent organisms attached themselves to the shells of the mollusca.

Although marked changes are observable in the character of the deposits forming the Gault series in Kent, where they consist of green-sands, clays, and marls, it is somewhat remarkable that the actual rhizopodal fauna does not greatly vary; and so far as one can judge from the results now before us, the depths were not subject to so much oscillation as the lithological character of the beds might at first sight seem to demand. They are all more or less comparable with deposits forming in the moderately deep seas of the present day: they are probably represented by the green, blue, and red muds and the green-sands for the Lower Gault; and by the semi-pelagic or terrigeno-*globigerina* ooze (the meeting ground of the terrigenous and the pelagic deposits) for the marls of the Upper Gault.

In connection with the subject it may be remarked that some years ago Professor T. Rupert Jones stated, in a note on an annelid bed at Westwell Leacon (3) that his colleague, Professor W. Kitchin Parker, believed the Gault Sea to have been 100 fathoms.

F. G. Hilton Price believes the Gault Sea not to have exceeded 100 fathoms in depth, and probably much shallower (4).

In his book "The building of the British Isles" (5), A. J. Jukes-Browne says with regard to this question, "The clays of the Lower Gault seem to have been deposited in a shallow sea of 50 to 70 fathoms deep, which is about the depth of the sea between England and Ireland, while the fossils of the Upper Gault of Folkestone indicate a depth of 100 fathoms and upwards." From the foraminiferal data for each zone of the Folkestone Gault to be referred to subsequently, we obtain a mean depth for the Lower Gault (Zones I.-VII. of Price) of 830 fathoms. In a similar way the Upper Gault (Zones IX.-XIII.) gives a mean depth of 866 fathoms.

The following are the zones with their separate results and points of interest:—

Zone I. The Green-sand seam at the base of the Gault.—This is a dark-coloured argillaceous green-sand. The included fossils are much rolled and worn, and this particular deposit appears to have been subjected to the prolonged action of currents. The depth obtained by the evidence of the foraminifera of this bed appears somewhat great, but can be accounted for by the fact of there being a later foraminiferal fauna present, besides the assemblage of glauconite casts. This mixed fauna also appears again in Zone XII. of the Gault (formerly referred to as Zone XI. green-sand seam).

For Zone I. basal bed a possible depth of 750 fathoms is obtained.

At this horizon a single example of *Hormosina globulifera* was found, and although usually occurring at greater depths, it is interesting to note that Dr Goës records it from a depth nearly corresponding with the determination given above.

The samples of green-sand collected during the voyage of the 'Challenger' were taken from depths less than 900 fathoms, the average being 449 fathoms. With regard to the hydrographical distribution of green muds and sands, Messrs Murray and Renard observe in the volume on "Deep Sea Deposits," p. 240, that they "would appear to form an interrupted band along many continental shores at the upper edge of the continental slope."

The usual sequence of the shallow to the deeper parts of the areas occupied by terrigenous deposits is in the order of green-sand (where conditions for its formation are favourable), green mud and blue mud. In the case of the Gault this was followed by a semi-pelagic deposit forming the grey marls of the Upper Gault.

From the samples of green-sand obtained by the 'Challenger,' one may refer, for comparison, to the green-sand, Station IV., between Cape St Vincent and Gibraltar, depth 600 fathoms.

Zone I. 5 feet above the base.—A dark clay, greenish when wet, bluish when dry.

This is probably equivalent to the modern fine glauconitic muds.

In this clay there is a fair quantity of minute glauconite grains, found only in the finest washings. The foraminifera yield evidence of slightly shallower conditions than the preceding, which, however, is probably placed at too great an estimation, since the foraminifera obtained from the green-sand seam, as previously pointed out, are not numerous enough to be representative.

The depth for this deposit is 700 fathoms. For comparison, one may refer, as a typical green mud of similar depth, to 'Challenger,' Station No. 163 F., off Sydney, depth 650 fathoms.

Zone II.—The samples taken from this zone were clays of a dark green colour.

The residua yielded a fair quantity of glauconite, and the presumably pelagic *Globigerina cretacea* was met with in some frequency. In their general character these clays are comparable with the green muds.

The depth of these samples works out at 820 fathoms.

Zone III.—The clay of this zone is of a pale brown or fawn colour, and is quite distinct in appearance from the rest of the Gault. Glauconite is extremely rare, and appears to be entirely absent in the modern red muds, with which this clay may perhaps be compared. The comparison, however, is not much more than one of similarity of colouration in its present condition, for much of the colour in modern red muds is due to ochreous matter, whilst that of the Gault of this zone is due to carbonate of iron with some ochreous staining. It is, however, not very probable that it could have originally been a blue mud, since this would have resulted, as with some other samples of the Gault, in the infilling of the foraminiferal shells with pyrites instead of carbonate of iron, of which we here have evidence. This carbonate of iron is found in some quantity disseminated through the clay as minute casts of organisms; and there are also concretionary bands of the same material running through the bed. This concretionary iron band is perforated throughout with what are apparently annelid borings, and this has been noticed by Mr Hilton Price (5), who drew attention to it in 1876 in connection with a similar annelid bed which Professor Rupert Jones had described (3) from Westwell Leacon in Kent, and locally known as 'Harper.' This bed Professor Jones found in the "upper part of the lowest third of the Gault" at that locality; and he compared it with a bluish-grey mud with annelids found forty miles S.E. of No-Sima Lighthouse, Japan, at a depth of 1875 fathoms. Professor Jones has been good enough to give me a specimen of the annelid rock from Westwell Leacon, and I find it comparable with a similar bed which I found some years ago in the Gault at Godstone. All three specimens probably occur at about the same horizon of the Gault and are equivalent to Zone III. at

Folkestone. The specimens from Westwell Leacon and Godstone are still clays, with no colouration, the tubular infillings being bluish grey, whilst the Folkestone specimen is a clay ironstone.

It is noteworthy of Zone III. at Folkestone, that owing to the absence of pyritous infilling of the foraminiferal shells, they are much paler in colour than is usual with Gault specimens of foraminifera.

The conditions in this zone seem to have been favourable for the crustacea, and by their abundance this bed is known to collectors of fossils as the 'Crab bed.' At this horizon Ostracoda are especially abundant. *Globigerina cretacea* also forms a fairly large proportion of the washings.

From the evidence of the foraminifera, the depth of this deposit was 1180 fathoms.

The character of the clay makes it appear to have been originally a red mud of a semipelagic nature.

Zone IV.—The clay of this zone is greenish grey. The foraminifera are very minute, owing to the prevalence of muddy matter, and the scarcity of dissolved calcareous material.

This clay is probably represented in modern deposits by blue mud rather than green mud, since glauconite, although present, is only in a small proportion in the washings.

The depth determined for this zone is 840 fathoms.

Zone V.—A grey-blue clay spotted with lighter markings. The washings contained a large proportion of *Globigerina cretacea*, *Sphaeroidina bulloides* also being found.

This deposit seems originally to have been a blue mud, from the quantity of pyrites found infilling the foraminiferal shells and elsewhere, and from the small quantity of glauconite present.

The depth determined by the foraminifera is 750 fathoms.

Zone VI.—This is a mottled blue-grey clay. There is much pyritous material as in Zone V. The deposit appears to be equivalent to the blue muds.

The probable depth is 790 fathoms.

Zone VII.—A dark blue-green clay, which from the scarcity of glauconite and the presence of pyrites, must have originally been a blue mud. This and the next zone above seems to have been formed under conditions particularly favourable for the existence of the redundantly grown *Pulvinulina spinulifera*.

The depth for this zone is 810 fathoms.

Zone VIII.—A grey clay, with a little glauconite in the fine washings. This also can be classed with the blue muds. The ferric carbonate casts become scarcer from Zone III. upward, and are entirely absent in this zone, so far as I have observed, but they again recur in the next and succeeding zones.

The evidence of the foraminifera points to 700 fathoms for the depth of this deposit.

Zone IX.—A dark blue-grey marl. *Globigerina cretacea* is found in some abundance, and from thence through the succeeding zones increases in quantity until near the top of the Gault. The deposit, compared with recent accumulations, might be termed a grey terrigenous ooze.

The foraminifera indicate a depth of 910 fathoms.

Zone X.—A pale green-grey marl. This zone perhaps more nearly foreshadows conditions which obtained in the Chalk-marl than any of the others. The proportion of calcareous matter is very large (as much as 45%); at 45 ft. from the top—in Zone XI. it was 36%; the Chalk-marl of Eastwear Bay at 10 ft. above the 'Chloritic' marl gave 67½% of calcareous matter. The conditions existent then in Zone X. must have been favourable for calcareous shelled organisms. Here particularly we obtain a great variety of the strong shelled and costate forms of the genera *Nodosaria*, *Frondicularia*, *Marginulina*, and *Vaginulina;* and it was from this zone more especially that the redundant and abnormal forms described in my systematic papers (see Part X. Foraminifera of the Gault of Folkestone) were obtained. This deposit may be classed with those of modern date as a grey terrigenous ooze, and had a probable depth of 900 fathoms.

Zone XI.—This bed, measured up to the base of the green-sand seam, is a pale grey marl. *Globigerina cretacea* considerably increases in abundance, and attains its maximum profusion at 45 ft. to 25 ft. below the top of the Gault, as well as in the next zone at 20 ft. from the top.

This bed can be compared with a grey terrigenous ooze, and appears to have been deposited in 870 fathoms.

Zone XII.—A glauconite-marl. A noteworthy point about this deposit is that the glauconite casts have been formed at a less depth than that at which the associated foraminifera lived; for the foraminiferal tests seen intermingled with the glauconite casts in the washings undoubtedly belong to a later period than the originals of the casts themselves; these remarks also apply to the microzoic fauna of the green-sand seam of Zone I.

This deposit is to some extent comparable with the glauconite muds, and its depth is indicated as 820 fathoms.

Zone XIII.—A pale grey marl, perhaps to be compared with the grey terrigenous oozes of modern deposits.

The foraminifera indicate a depth of 830 fathoms.

It is here necessary to refer to a few points in explanation of the evidence afforded by the foraminifera alone, as regards the depth of sea in which these organisms lived.

It has already been pointed out that those authors who have expressed any opinion as to the depth of the Gault Sea have not given anything like the depths shown by these results based upon a systematic inquiry into the distribution of the foraminifera throughout the Gault at Folkestone (2). Previous authors, with the exception of Professors Parker and Jones,[1] have based their results upon data afforded by a consideration of the groups of the mollusca, crustacea, and other of the larger organisms. These higher groups from modern deposits have, in many cases, only been specially dredged from moderately shallow depths. Although the bathymetrical range of these larger forms is in most cases rather limited to the shallower parts of the ocean, it appears to me extremely probable that current action, of which there is abundant proof throughout the Gault at Folkestone, has there operated in bringing together assemblages of testaceous remains from the higher continental slope on which they flourished, to greater depths where these accumulations took place. It is more reasonable to imagine the removal of the shallow forms to deeper areas than to suppose that the finer muds with foraminifera could be brought into shallower waters.

The presence of phosphatic nodules, so abundant in the Gault, by no means indicates shallow water. That these are due to currents, and by the changes of temperature consequent on their intermingling, has been clearly shown by Murray and Renard ("Deep Sea Deposits," p. 397), who state that phosphatic concretions "may be found in all terrigenous deposits, and also along the edge of the abyssal zone in deposits of a pelagic type, which, however, from their nearness to land, still contain terrigenous elements." These authors also point out (p. 396) "that phosphatic nodules are apparently more abundant in the deposits along coasts where there are great and rapid changes of temperature, arising from the meeting of cold and warm currents, as, for instance, off the Cape of Good Hope and off the eastern coast of North America. It seems highly probable that in these places large numbers of pelagic organisms are frequently killed by these changes of temperature, and may in some instances form a considerable layer of decomposing matter on the bottom of the ocean."

That current action played an important part during the deposition of the Gault is, therefore, not only proved by the numerous lines of phosphatic concretions found at certain intervals, but also by the presence of green-sand seams and scattered glauconite grains found throughout the formations.

The depths here given for each zone of the Gault are merely recorded for what they may be worth; for after all it is a result

[1] Professor Rupert Jones has already expressed to me his belief that the calculation made many years ago by him and his colleague Parker is probably of far less depth than it should be.

obtained from a consideration of only a small proportion of the rhizopodal fauna,—that which is represented in our modern seas.

Summary of Details respecting the Gault and its Depths.

Horizon.	Nature of Deposit.	Possible Equivalent in Deep-Sea Deposits.	Average Depth in Fathoms.
Zone I. (Green-sand seam).	Dark argillaceous green-sand.	Green-sand.	750
,, I. (5 ft. above the base).	Dark green clay.	Green mud.	700
,, II.	,, ,, ,,	,, ,,	820
,, III.	Pale brown clay.	Red mud.	1180
,, IV.	Green-grey clay.	Blue mud.	840
,, V.	Grey-blue clay.	,, ,,	750
,, VI.	Mottled blue-gray clay.	,, ,,	790
,, VII.	Dark blue-green clay.	,, ,,	810
,, VIII.	Grey clay.	,, ,,	700
,, IX.	Dark blue-grey marl.	Grey terrigenous ooze.	910
,, X.	Pale green-grey marl.	,, ,, ,,	900
,, XI.	Pale grey marl.	,, ,, ,,	870
,, XII.	Glauconite marl.	Glauconite mud.	820
,, XIII.	Pale grey marl.	Grey terrigenous ooze.	830

Frederick Chapman.

111 Oakhill Road,
Putney, S.W.

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598.3
591.156

II

The Gular Pouch of the Great Bustard (*Otis tarda*)

IN reviewing the history of the gular pouch of the Great Bustard we have of necessity to trace the history of the explanation of two very different phenomena, which at last resolve themselves into complementary halves of a common whole. The first of these deals with the fact now known to all ornithologists, that several different species of Bustard have the power of inflating the neck to an enormous degree, at intervals during that period when, as the poet has it, "fancy, lightly turns to thoughts of love." It is one of the many methods of 'showing off' to be found in such abundance amongst birds. At least three different versions have been given to explain how this inflation is brought about. The second, as already hinted, is linked with that of the first. It concerns what is the main theme of this paper,—the Gular Pouch. The very existence of such a structure has been denied by some, by others it has been held to be a receptacle for water, food, and air. Those who subscribed to this latter view, for the most part connected it more or less definitely, with the curious love displays just referred to, and knew something of the habits of the living birds, which the others did not. The aim of the present paper is to give a sketch of these various conflicting interpretations and to draw attention to one or two minor points around which some doubt still seems to hover.

The earliest known indication of the possession of this faculty of inflating the neck by the Great Bustard dates back as far as 1681. This we owe to Sir Thomas Browne[1]: he remarks that "as a Turkey hath an odde large substance without, so had this [*Otis tarda*] within the inside of the skinne." Here however we have nothing more than a bare statement drawing attention to the fact that the neck of this species of Bustard differed from that of birds generally in this respect, and we are left to imagine that it is a constant character possibly possessed by both sexes in common. Some half century later a real contribution to our knowledge of the subject was made, which was destined to become the subject of much animated discussion. It concerns the gular pouch. This we owe to Dr James Douglas, a British anatomist. The first mention of this was made by Albin in 1740, for Douglas it seems did not

[1] The quotations from the earlier writers are taken for the most part from Professor Newton's valuable article in the *Ibis* for 1862.

announce his discovery during his lifetime. Albin's reference runs as follows: "Dr Douglas has observed in the Male [of the Great Bustard] two Stomachs, one for the Food and the other a Reservatory for Water to supply them, they feeding in dry Heaths remote from Ponds and Rivers." A fuller account was given by Edwards in 1747. This also deals with the discovery of Douglas, and repeats the interpretation originally given, of the use of the pouch as a receptacle for water. "Its capacity," he says, "is full seven Wine Pints." It is further stated to be wanting in the Hen.

M. Gauthier de la Peyronie, in his "Voyages de Pallas" (15a) says of the Great Bustard:—"Cet animal a un petit trou sous la langue, qui sert d'ouverture à une bourse aqueuse, qui est de la grosseur d'un oeuf d'oie." That the Great Bustard—and probably also other species—possessed a gular pouch which was used as a receptacle for water seems by this time to have been pretty generally believed. Thus in 1781, Daines Barrington tells us of "a gentleman long resident at Morocco, where they frequently fly their hawks at bustards, hath also informed me that the cock makes use of this reservoir of liquor against these assailants, and commonly thus baffles them." In the following year (1782) Bloch gave an account and figure of this pouch. He remarks: "Bey diesem grossem Vogel * * * siehet man einen Sack unter der Haut am Halse, dessen Oefnung unter der Zunge sichtbar ist. . . . Er ist weit, war bey einem alten Hahn, den ich untersuchte ein Fusslang, und erstreckte sich von der Kehle bis an die Brust." He then goes on to remark, "dass nur die Männchen allein mit diesem Sack versehen wären, so widerspricht diesem meine Erfahrung; denn ich habe ihn auch bey einen Weibchen gefunden." This is the first statement of its occurrence in the female, and one, we may remark, which has never yet been verified.

Naumann in 1834 gives the results of his observations on this subject. After describing the general position of the pouch, and its opening under the tongue, he goes on to remark that "Er hat, wenn er mit Luft oder Wasser angefüllt ist, oft eine einfache, sehr langgezogene Eigestalt; gewöhnlicher noch ist er aber am Eingange enge; dann eiförmig erweitert und in der Mitte seiner Länge am weitesten; nachher wieder sehr verengert; dann wieder in Eiform, aber kürzer und nicht so stark wie oben erweitert und wie ein spitzes Ei geschlossen. . . . Er fasst eine ziemliche Menge Wasser, doch lange keine 8 Pfund, und man vermuthet, wiewohl ohne Grund, er sei ein änlicher Wasserbehälter wie der des Kameels, um Vorrath trinken zu können; aber warum war er denn dem Weibchen nicht auch gegeben?! Wasser fand ich überhaupt darin nur sehr wenig, nicht einmal einen Esslöffel voll, vielmehr ihn meistens ganz leer, nur ein Mal einige Grassamen, welche zufällig

hinein gerathen zu sein schienen. Er scheint mir überhaupt mehr ein Luftals Wasserbehälter zu sein. Sein Zweck bleibt vor der Hand ein Räthsel, wie er dies schon lange war."

John Hunter apparently made a dissection of this pouch. He did not, however, subscribe to the prevalent opinion that it served as a receptacle for water, on the contrary he candidly expresses that he does not know what its use may be. He describes it as "a large bag, as large as the thick part of one's arm: it terminates in a blind pouch below, but has an opening into it at the upper end from the mouth. This aperture will admit three or four fingers[1]; it is under the tongue, and the fraenum linguae seems to enter it; and it seems to have a sphincter. What the use of this is I don't know. . . ."

None of the writers so far quoted seem to have connected this pouch with the phenomena of sexual display, probably because they had never witnessed the remarkable evolutions and contortions of this bird during its moments of ecstatic frenzy.

The credit of this interpretation perhaps belongs to Schneider. It seems, however, that he had never examined this pouch for himself, but relied on the accuracy of the observations of those more fortunate who had. Commenting upon Bloch's statement, which evidently much puzzled him, that the pouch was found in both sexes, he says:—"Si mas solus sacco gulari gaudet, potest tum in amore eum forte inflare, ut collum intumescat. Contra si femina eundem habet, quod vix credo, alium tum eidem usum excogitare debemus." It is possible, however, that Schneider is indebted to the Emperor Friedrich the II. for the suggestion that the pouch may be occasionally and voluntarily inflated. Inasmuch as the latter, acquainted only with the external phenomena, drew attention to the "'grossum collum' possessed by both sexes of the Great Bustard, and especially the males 'tempore coitûs.'"

Quite another rendering was given as an explanation of this curious inflation of the neck by Degland (4), who writes:—"Je dois à mon honorable confrère, le docteur Dorin, de Châlons-sur-Marne, la connaissance d'un fait assez curieux et que je ne dois pas omettre. A l'époque des amours, il se développe dans le lieu même où s'insèrent les moustaches, une sorte de fanon, formé par une masse de tissu cellulaire graisseux, lâche, dont le volume est considérable, puisqu'il atteint et dépasse le poids d'un kilogramme. Cette sorte de fanon, qui occupe la partie antérieure et latérale du cou, est formée de deux masses qui se réunissent sur la ligne médiane à partir de la naissance des barbes jusqu'au bas du collier. C'est au moyen de muscles fauciers assez développés que l'oiseau peut imprimer des mouvements à cette masse, et par conséquent relever ou abaisser les

[1] The spaced type is mine.

plumes allongées qui s'y implantent. A la fin de juillet, elle commence à s'affaisser, les plumes tombent, se renouvellent, si bien qu'avant la fin de septembre il ne reste plus rien de cette grande masse de tissu cellulaire." Owen, a year previously, had dissected a specimen, said to have been a male,[1] apparently for this purpose, and found "no trace of a gular pouch," thus so far confirming Degland: Mitchell, Yarrell, and later, Professor Newton, all searched carefully for this pouch, and failed to find it; neither could they discover any opening under the tongue. The latter thus describes his search:—"We cleared the skin away from the entire neck. . . . The neck was entirely clothed with cellular tissues in a most remarkable manner; they were very delicate, and so close to the skin, that even when we grazed the roots of the feathers we occasionally cut them. On the blowpipe being inserted into one of the apertures thus made, a small bubble was immediately raised, which increased on greater power, being applied so as to form a considerable bag, perhaps three inches long, . . . it was plain . . . that none of these bags existed of themselves, but were the result of the membranes being forcibly ruptured by the pressure of the air."

Thus, then, at this time, so far as English ornithologists were concerned, the case for the existence of a gular pouch in the Bustard had fallen through for lack of evidence. There seemed to be no other way of explaining the facts advanced by the older writers than that of supposing the 'pouch' which they saw was artificial, caused by the rupture of cellular tissues. Unless indeed it was, as some suggested possible, present in some individuals, but not in others. That the specimens dissected in England, says Professor Newton (14), "were not all young, undeveloped birds, is also clear; but if any further evidence on this point is required, I would refer to the beautiful picture by Mr Wolf (fig. 1), which was drawn from an individual in our Zoological Gardens,—an individual afterwards the subject of one of the examinations here mentioned, though of which is not certain. No one who looks at that picture . . . can for a moment doubt that the original was a truly adult, mature, and fully developed bird.

Dr Cullen (3), inspired by Professor Newton's article (14), published the results of an examination of two males procured by him in Kustendjie, Bulgaria. In both of these a pouch was found, the largest of which he figured. The "opening under the tongue," he writes, "is large enough to admit readily the little finger, and is surrounded by what has all the appearance of a sphincter-muscle . . . the pouch extended as far down as the furcular bone, enveloped closely throughout by a thin muscular covering exactly analogous in structure to the *cremaster* or *platysma hyoides*. The structure of the sac . . . is certainly not composed of cellular tissue as stated by

[1] Garrod suggested that this was probably a female.

Degland; but . . . is a separate and distinct, though delicate bladder. . . . After describing the very extraordinary evolutions of this bird during periods of display, and the great inflation of the neck with which they are accompanied, he goes to remark that "All these facts would certainly seem to favour the idea that the pouch is intended to contain air, and that by the action of the muscular tissue covering it conjointly with that of the sphincter at the mouth, the Bustard may thereby be assisted . . . in producing the peculiar sound (resembling 'ook'), which is only to be heard during the time when the pouch is most developed. . . ."

FIG. 1.—The display of the Great Bustard, *Otis tarda* (after Wolf). From Newton's "Dict. of Birds."

These two preparations afterwards came into the possession of the Royal College of Surgeons, and were described by Sir William Flower (6). The larger of the two sacs when empty measured nine inches in length, and when moderately distended with water was found to hold three imperial pints.

"Both of the sacks," he writes, "had within them a few short pieces of grass and leaves. There appears to be no glandular structures connected with the walls; indeed, the whole character of the sack points to its being a simple reservoir, probably for fluid, more analogous to the submandibular pouch of the Pelican than to anything else in the class Aves. But in the absence of fuller information as to the economy and habits of the bird, I refrain from speculating upon the purpose of this singular and apparently inconstant organ."

With Dr Cullen's investigations and their confirmation by Professor Flower the existence of a pouch at least in some individuals was placed beyond cavil.

In 1868 Dr Murie, then Prosector of the Zoological Society, published his "Observations on the presence and function of the Gular Pouch in *Otis kori* and *Otis australis*." His remarks concerning the former were based upon the examination of a specimen which had recently died in the Gardens. In this, a distinct opening was found under the tongue leading into a small pouch "three inches in length and about an inch in transverse diameter." As touching the latter, his observations were based entirely on a bird then living in the Society's Gardens. Of this he writes that he was pleased to find what he thinks "may be termed an exaggerated example of this organ in the Australian Bustard." He continues: "This 'showing off' which is . . . a most extraordinary sight, may best be comprehended by a study of the accompanying sketch (fig. 2) drawn from nature during one of those paroxysmal periods of excitement."

FIG. 2.—The display of the Australian Bustard, *Eupodotis* [*Otis*] *australis* (after Murie). [This block was reproduced from a photograph of a coloured lithograph, and is unfortunately not very clear.]

"The premonitory symptoms observable when the Bustard is about to exhibit himself in the pride of lust . . . is a slight swelling of the inframandibular portion of the throat, while the head is thrown upwards. Immediately afterwards the neck swells and the feathers of the lower parts concomitantly bulge out and descend gradually downwards in the form of a bag, oftentimes nearly reaching the ground.

"If the paroxysm is a strong one, then the tail is shot upwards and forwards over the back, the rectrices coming almost in contact with the neck.

"In this peculiar attitude, with bloated neck, hanging baggy chest, elevated tail, and stiff stilt-like legs, the creature struts about in a somewhat waddling manner, the elongated pouch swaying to and fro.[1] The feathers of the throat start out on end; those of

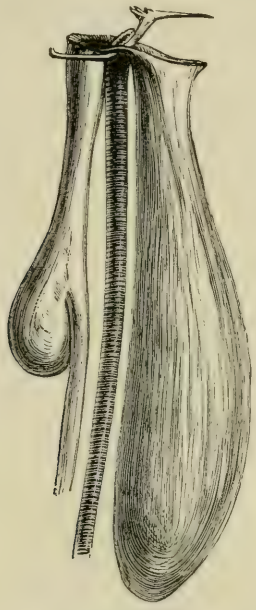

FIG. 3.—The oesophagus, trachea, and gular pouch of a specimen of *Otis tarda*, seen from the side. The crop is here drawn as in the actual preparation, projecting backwards, and not forwards as usual. (After Garrod.)

the depending sac are also raised, but less upright. While all this has taken place the bird seems to have gulped in air, or rather, with partly opened gape, to have taken a long, deep and forced inspiration.

"The acme of inspiratory effort and strange attitude attained, the Bustard begins to snap the mandibles together in a loud manner and utter a series of cooing sounds for a short interval of time. Usually and more frequently he struts towards the female Bustards in a most dignified manner, or oblivious as to sex, totters up to any of the birds in the same enclosure."

Some years later (in 1873) the mouth of this identical Bustard was examined by Professor Garrod,—Dr Murie's successor to the Prosectorship,—with a view to finding a sublingual orifice such as obtains in *O. tarda*. There was no trace of any such orifice. This

[1] The spaced type is mine.

led Garrod " to doubt the correctness of Dr Murie's inference, that because the neck of *Eupodotis australis* becomes distended much during the sexual season, therefore there is a gular pouch." The next year this bird died and was dissected by Garrod. As a result, " there was no gular pouch. There was no sublingual orifice. . . . How unsafe therefore is it to infer that because the neck distends and depends during the ' show-off,' there must be a sublingual pouch. It is quite possible that two effects, very similar in appearance, in closely allied birds, may be the result of different mechanisms." A careful investigation showed that the cause of the inflation of the

FIG. 4.—The oesophagus and trachea of the specimen *Eupodotis australis* here described. The oesophagus is much dilated, and, like that of the Pouter Pigeon, can be distended with air by the living bird. No trace of a pouch or crop is to be seen (after Garrod).

neck in this case was due to a highly extensible oesophagus. " Before dissection, by filling its cavity with air, the lower portion of the dilated oesophagus protruded downwards considerably in front of the *symphysis furculae*, and formed the depending portion of the sac which was so conspicuous in the living animal." The two woodcuts (figs. 3 and 4) kindly lent by Mr Sclater for the present paper, are taken from Garrod's original paper.

Fig. 5 represents a dissection which the writer has just made of the gular pouch of an adult male till recently living in the Gardens of the Zoological Society; and which will shortly be exhibited in

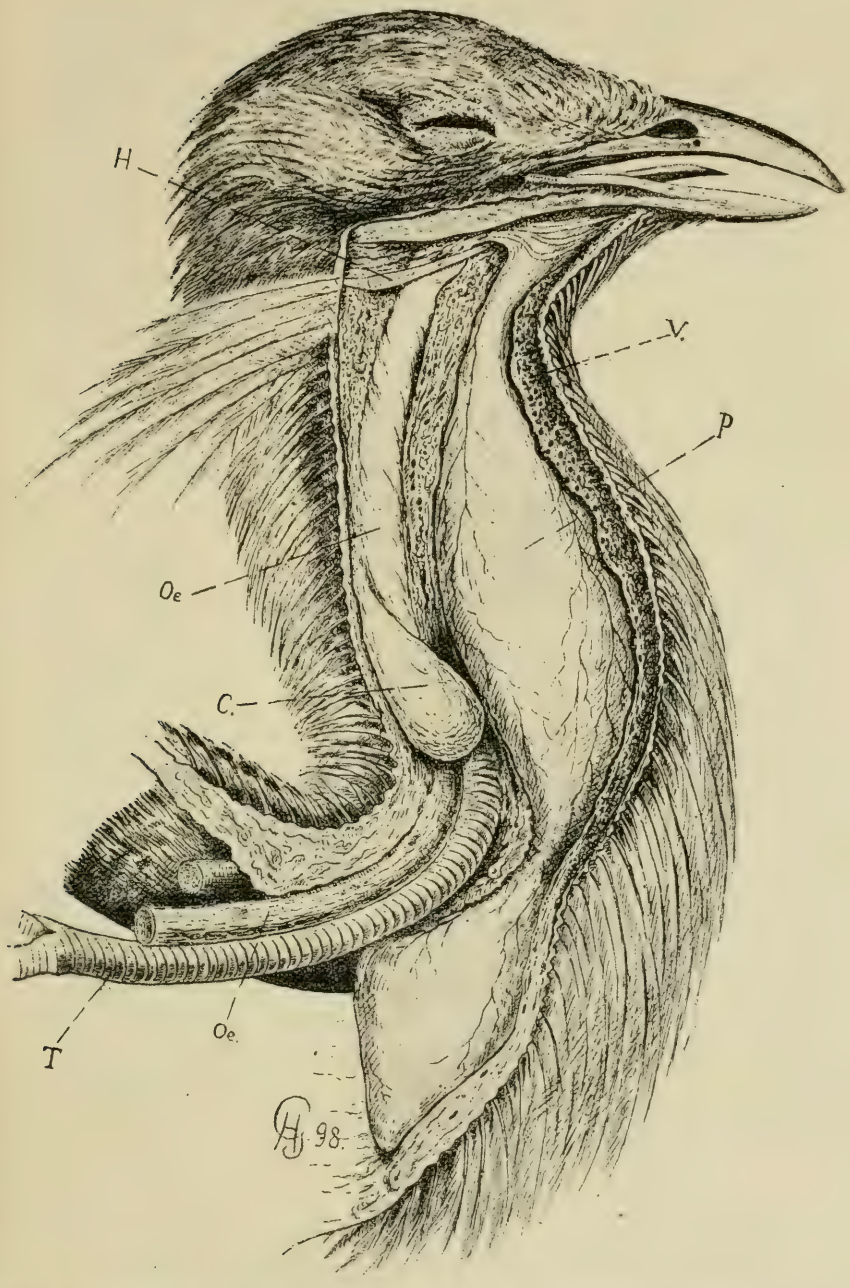

FIG. 5.—Dissection of the right side of the neck of the Great Bustard, *Otis tarda*, to show the hour-glass-shaped gular pouch. [Drawn by H. Grönvold from the specimen in the British Museum.] *C.* Crop; *H.* Hyoid; *Oe.* Oesophagus; *P.* Pouch; *T.* Trachea; *V.* Vascular tissue, investing the upper part of the pouch.

the Bird Gallery of the British Museum (Natural History). There is no need to describe in detail its form, capacity, and so forth; particulars of this kind will have been gathered already from the preceding pages. I might, however, remark that the sublingual aperture in my specimen was not ⊥-shaped but circular showing a hole large enough to admit the finger. Possibly this was due to relaxation of the muscles. After removal of the head and neck, the pouch was filled with spirit till it overflowed, the whole was then plunged into 70 per cent. spirit and left for some days. It was then taken out, and the skin from one side removed (fig. 5). Underlying was a mass of fatty tissue more or less completely investing the pouch. Along the anterior aspect of the neck, from the throat downwards, this is engorged with blood. The pouch was loosely attached to this investiture by delicate strands of fibrous tissue. The constriction in this pouch probably corresponds with that described by Naumann, and occurs at the lower part of the neck where it bends between the furcula, between the arms of which the expanded terminal portion is received.

In conclusion I would point out:—(1) That the characteristic 'show-off' of the adult male *Otis tarda* ever takes place without the aid of a gular pouch, is exceedingly improbable. But that this pouch is present throughout the year is another question, and is a point which has yet to be settled. It was not found in the specimen from which Wolf's beautiful drawing was taken, nor in numerous other cases in which it was carefully searched for. In the specimen lately dissected by myself, it was, as is shown in the illustration (fig. 5), very large. But this bird died in May, in the middle of its period of functional activity; (2) There is no evidence to show that it is ever present in the female; (3) The belief that this sac is ever used as a receptable for water must now be regarded as utterly exploded; (4) It is not homologous with the 'air-sacks' proper, belonging neither to the pulmonary nor to the naso-pharyngeal system. *Biziura lobata* seems to be the only other bird besides the Bustards possessing a precisely similar structure.

W. P. Pycraft.

British Museum (Natural History),
S. Kensington, London.

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III

An Existing Ground-Sloth in Patagonia[1]

MANY times I have heard allusions to a mysterious quadruped which is said to exist in the interior of the territory of Santa Cruz, living in burrows hollowed out in the soil, and usually only coming out at night. According to the reports of the Indians, it is a strange creature, with long claws and a terrifying appearance, impossible to kill because it has a body impenetrable alike to fire-arms and missiles.

It is several years since the late Ramon Lista, a traveller and geographer well known to the world of science, told both myself, my brother Charles, and several other persons—and had, I believe, even printed the statement in one of his works—that he had seen the mysterious quadruped in question. He came across it one day during one of his journeys in the interior of the territory of Santa Cruz, but in spite of all his efforts he was unable to capture it. Several shots failed to stop the animal, which soon disappeared in the brushwood; all search for its recovery being useless.

Lista retained a perfect recollection of the impression this encounter made upon him. According to him the animal was a pangolin (*Manis*), almost the same as the Indian one, both in size and in general aspect, except that in place of scales, it showed the body to be covered with a reddish grey hair. He was sure that if it were not a pangolin, it was certainly an edentate nearly allied to it.

In spite of the authority of Lista, who, besides being a learned traveller, was also a skilled observer, I have always considered that he was mistaken, the victim of an illusion. Still, although I have several times tried to find out what animal might have given him the illusion of the pangolin, I was never able to guess.

It was not an illusion. Although extremely rare and almost extinct, the mysterious animal exists, with the sole difference, that instead of being a pangolin, it is the last representative of a group

[1] Translated from a pamphlet entitled "Premiére Notice sur le *Neomylodon listai*, un Représentant vivant des anciens Edentés Gravigrades fossiles de l'Argentina," by Florentino Ameghino, separately published by the author in the city of La Plata, Argentine Republic, August 1898. We have already noticed this important discovery (p. 288), but it is one of so much interest to zoologists that no apology is needed for directing further attention to the subject by reproducing the complete article.

which was believed to be quite extinct, a gravigrade edentate related to *Mylodon* and *Pseudolestodon*.

The gravigrade edentates are reckoned among the oldest mammals which appeared upon the earth. The most ancient traces of them have been observed below the Guaranian Formation, with gigantic Dinosaurs, in the variegated sandstones of Patagonia, which are referred to the Lower Cretaceous. They become more numerous in the *Pyrotherium* beds of the Guaranian, develop gradually, and attain their greatest diversity during the Upper Eocene (Santa Cruz Formation). Thenceforward their variety decreases, but their size gradually increases, until in the Pampean they are represented by a certain number of gigantic forms, such as *Megatherium*, *Lestodon*, *Mylodon*, etc. Rare fragments in a bad state of preservation have been found even in the Post-Pampean deposits, but no one had supposed that they still had living representatives.

Some of the Pampean genera show a very curious character: the body was protected on all sides by an incredible number of small irregular ossicles, which it is supposed were developed in the thickness of the skin, and thus became covered with a horny or scaly epidermis. The genera showing this peculiarity are *Mylodon*, *Pseudolestodon*, and *Glossotherium*. The other genera, such as *Megatherium*, *Lestodon*, and *Scelidotherium*, do not show any trace of it. Besides in the Pampean Formation these ossicles are met with in the Araucanian Formation of Monte Hermoso and Catamarca, and also in the Entrerios Formation; but no trace of them has been found in the Santacruzian, where the gravigrade edentates are so abundant, or in the earlier formations. We conclude from this that the character in question is not primitive, but acquired secondarily at a relatively modern period.

These ossicles, comparable to large coffee berries, differ slightly in shape and size according to the genera. In *Glossotherium* they are large and flattened; in *Mylodon* they are smaller, irregular, elliptical, trapezoidal, or rhomboidal, with one side more convex or keeled, their diameter varying from one to two centimetres, though sometimes less. Their surface, more especially on the flattest side, shows some tiny depressions and perforations, and reticular tracery well seen under the magnifying glass. Their aspect is so characteristic that when one has once seen them they are recognised immediately without any danger of being mistaken.

Lately, several little ossicles have been brought to me from Southern Patagonia, and I have been asked to what animal they could belong. What was my surprise on seeing in my hand these ossicles in a fresh state, and, notwithstanding that, absolutely similar to the fossil dermal ossicles of the genus *Mylodon*, except only that they are of smaller size, varying from 9 to 13 or 14 mm.

across. I have carefully studied these little bones from every point of view without being able to discern any essential difference from those found in a fossil state.

These ossicles were taken from a skin which was unfortunately incomplete, and without any trace of the extremities. The skin, which was found on the surface of the ground, and showed signs of being exposed for several months to the action of the air, is in part discoloured. It has a thickness of about 2 centimetres, and is so tough that it is necessary to employ an axe or a saw in order to cut it. The thickest part of the skin is filled by the little ossicles referred to, pressed one against the other, presenting on the inner surface of the skin an arrangement similar to the pavement of a street. The exterior surface shows a continuous epidermis, not scaly, covered with coarse hair, hard and stiff, having a length of 4 to 5 centimetres and a reddish tint turning towards grey.

The skin indeed belongs to the pangolin which Lista saw living. This unfortunate traveller lost his life, like Crévaux, in his attempt to explore the Pilcomayo, and until the present time he is the only civilised person who has seen the mysterious edentate of Southern Patagonia alive ; and to attach his name appropriately to the discovery, I call this surviving representative of the family Mylodontidae *Neomylodon listai*.

Now that there are certain proofs of its existence, we hope that the hunt for it will not be delayed, and that before long we may be able to present to the scientific world a detailed description of this last representative of a group which has of old played a preponderating part in the terrestrial faunas which have succeeded each other on South American soil. FLORENTINO AMEGHINO.

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IV

The Imperfection of the Geological Record

IT is now many years since Darwin first directed the special attention of biologists to the imperfection of the geological record. It was he who first satisfactorily marshalled the facts which prove that the discoverable fossils in the rocks can only give a very limited idea of the plants and animals which have tenanted the globe at different periods in its past history. He pointed out how small a portion of the earth had been geologically explored, and how small a percentage of known types of life had sufficient hard parts to be preserved in a fossilised state. He emphasised the fact that the number both of specimens and of species preserved in our museums, is absolutely as nothing compared with the number of generations which must have passed away even during a single geological formation. He also observed " that, owing to subsidence being almost necessary for the accumulation of deposits rich in fossil species of many kinds, and thick enough to outlast future degradation, great intervals of time must have elapsed between most of our successive formations ; that there has probably been more extinction during the periods of subsidence, and more variation during the periods of elevation, and during the latter the record will have been least perfectly kept ; that each single formation has not been continuously deposited " ; that, indeed, in every area of the earth's surface there are incalculable periods of geological time unrepresented in the records of the rocks.

We may, in fact, without exaggeration declare that every item of knowledge we possess concerning extinct plants and animals depends upon a chapter of accidents. Firstly, the organism must find its way into water where sediment is being deposited and there escape all the dangers of being eaten ; or it must be accidentally entombed in blown sand or a volcanic accumulation on land. Secondly, this sediment, if it eventually happens to enter into the composition of a land area, must escape the all-prevalent denudation (or destruction and removal by atmospheric and aqueous agencies) continually in progress. Thirdly, the skeleton of the buried organism must resist the solvent action of any waters which may percolate through the rock. Lastly, man must accidentally excavate at the precise spot where entombment took place, and someone must be at hand,

capable of appreciating the fossil and preserving it for study when discovered.

The importance of remembering these considerations when speculating on biological subjects has recently been illustrated once more by the discovery of a new Upper Silurian fish-fauna in the south of Scotland. As already mentioned in *Natural Science* (vol. xiii., p. 157) this remarkable assemblage of fishes or fish-like organisms has been found by the Geological Survey at the top of the Silurian formations of Lanarkshire; and some preliminary notes by Dr Traquair announce that a complete memoir on the subject will shortly appear. Now, scattered and abraded fragments of similar organisms have been known for nearly sixty years in a thin stratum, termed the Ludlow Bone-bed, in the Upper Silurian of Herefordshire and adjoining counties. Fossils of the same kind have been collected for nearly half a century in an Upper Silurian limestone in the island of Oesel, in the Baltic Sea. Traces of them also occur in Galicia, Pennsylvania, and New Brunswick; and a few years ago similar fragments were sent to me by the Geological Survey of Canada from another locality in Newfoundland. However, notwithstanding this proof of the very wide distribution of the late Silurian fish-fauna in question, we have had to wait for the accidental discovery of a thin stratum in Lanarkshire to obtain even a faint idea of the strange types of life represented by the familiar scales and other exoskeletal fragments.

Although it is now nearly forty years since Darwin's "Origin of Species" first appeared, his lament at the hopelessness of testing all the principles of organic evolution by reference to the "records of the rocks" might indeed be appropriately renewed at the present day. The discovery of new fossils in all parts of the world has progressed at an astounding rate in the interval; and we are beginning to perceive feebly some of the laws which govern their succession and distribution. The biologist who is prone to glance through palaeontological text-books, however, and utilise them in his speculations, cannot be too frequently warned of the imperfection of our knowledge and the danger of trusting to negative evidence.

To understand the importance of this warning at the end of nineteenth-century science, it is only necessary to consider the case of some of the most striking and philosophically valuable vertebrate animals.

Firstly, there is the remarkably early Devonian organism *Palaeospondylus gunni*, frequently referred to in these pages. Whether it is a primaeval lamprey or not, it is the single known representative of its group, and implies the former existence of a great race of which we are acquainted with no other member. This fossil occurs in the Caithness flagstones, which were deposited in a

lake in the Devonian or Old Red Sandstone period, and have been worked for commercial purposes from time immemorial. Fossil fishes have been known and collected from them for more than seventy years. Every exposure has been searched by expert collectors, whether in the cliffs or in quarries. Yet, *Palaeospondylus* has only been found in one very thin stratum in a single quarry, where it occurs, not as a rarity, but in countless numbers. It seems as if a shoal of the species had been accidentally destroyed and suddenly covered up; and it is a fortunate accident that a small quarry has been opened at the precise spot.

Our knowledge of the earliest shark-like fishes exhibiting the most primitive type of paired fins is almost equally scanty. *Cladoselache*, in a state fit for accurate scientific study, has hitherto been met with only in a flagstone at the base of the Carboniferous formations in Ohio, U.S.A. Teeth of the same kind have been known for many years from several parts of the northern hemisphere; but the complete fish has only been discovered in Ohio within the last decade, and even now the skeleton of the head and vertebral axis remains practically unknown.

In some instances the old Palaeozoic forms of fish-life which withdrew to the comparatively peaceful realms of rivers and fresh-water lakes after the vigorous period of their race was past, and survived until the present day, were thus entirely lost to geological records. For example, three detached teeth from the English oolites and scarcely more from corresponding rocks in Colorado, are the sole known traces of the Dipnoan fishes between their world-wide distribution at the dawn of the Mesozoic era and the scattered remnants which still survive in the fresh-waters of South America, Africa, and Queensland. Similarly, there is no doubt that *Polypterus* and *Calamoichthys* existing in the fresh-waters of tropical Africa, are the direct and little-altered descendants of some of the Palaeozoic fringe-finned ganoids; but we have still not found even a trace of them in the Mesozoic or Tertiary strata in any part of the world. They must have lived somewhere, but the geological record, so far as explored, is too imperfect to afford a clue to their whereabouts and history.

The case of the Amphibia or Batrachia is still more remarkable; though perhaps they, too, have been fresh-water animals since the Palaeozoic era. It is definitely proved that some of the early lung-breathers belonged to this class; for traces of gill-arches are occasionally observed in young individuals, showing that they breathed by gills in their immature state (*Branchiosaurus, Archegosaurus*). It is also certain that these primitive Amphibia were the dominant type of vertebrate life from their appearance until the middle part of the Permian period. In early Mesozoic times, however, they suddenly

disappear from the records of the rocks; and practically nothing is known of them until the early Tertiaries, when the various genera and families are almost identical with those surviving at the present day. The only satisfactory specimen of intermediate date is a solitary skeleton (*Hylacobatrachus*) from the Wealden of Bernissart, Belgium, which seems to represent an animal with persistent gills related to the existing *Proteus* and *Menobranchus*.

The story of the early mammals is almost similar. In rocks dating back to the close of the Palaeozoic and the dawn of the Mesozoic period, there are abundant remains of the Anomodont reptiles or Theromora, which make an extremely close approach in their skeleton to the warm-blooded quadrupeds which we term mammals. They are found in South Africa, India, Russia, Switzerland, Scotland, North America, and South America. They must thus have been almost world-wide in their distribution. It is also clear that many of them attained a very large size. In all the regions mentioned, however, they completely disappear above the Trias; and the only known Mesozoic fossils which can be referred to the Mammalia are some fragments of animals no larger than rats from the Jurassic of England and the Jurassic and Cretaceous of North America. It seems, indeed, as if the mammals were evolved in some region of the southern hemisphere which is either now submerged or not yet geologically explored; for they suddenly appear in great numbers and variety at the base of the Eocene Tertiary both in Europe and North America, which must be the result of migration on the re-arrangement of land and sea. It is very curious that notwithstanding the numerous examinations of the Mesozoic and Tertiary strata of Australasia and South America during the last half-century, not a single clue to the solution of the problem has hitherto been obtained. As suggested by Mr Lydekker in a recent issue of the *Transactions* of the Geological Society of South Africa, it is extremely probable that we must turn to a geological exploration of the Dark Continent for the next important advance in our knowledge of the subject.

Our ignorance of the early land-mammals is strange, but the want of all knowledge of the ancestors of the marine mammals—whales, porpoises, and sea-cows—is still stranger. It is well known that at present all the great Mesozoic marine reptiles of the orders Ichthyosauria, Plesiosauria, and Mosasauria or Pythonomorpha, seem to disappear suddenly at the top of the Cretaceous formations; while the marine mammals of the orders Cetacea and Sirenia take their place as suddenly towards the top of the Eocene strata. This happens not only in Europe and North America, but also in Australasia, perhaps likewise in South America. Now, we are well acquainted with marine deposits, both of littoral and deep water origin, of intermediate age in many parts of the world.

Presumably, therefore, if the marine mammals are derived from terrestrial quadrupeds, as seems probable, we ought to find fossil records of their partially evolved ancestors in some of these deposits. As a matter of fact, we have hitherto found nothing. The earliest known Cetaceans and Sirenians are more nearly like normal land-mammals than the later and existing genera of the same orders; but the approximation is only very slight. They are completely differentiated on their earliest appearance, and the geological record, so far as explored, affords no clue whatever to their origin and affinities. It is, of course, possible that these aquatic animals originated during the Mesozoic period in some land-locked sheet of water or lake, of which the sediments have been destroyed or not yet discovered. Some American palaeontologists think it very probable that the seals originated in this way at an early Tertiary period in North America, where there were already great lakes. It may therefore be that the history of the other marine mammals is similar.

Not only is our ignorance deep and absolute in respect to many of these most fundamental problems: it also progresses very slowly even in some instances where enlightenment begins. Consider the case of the ancestral birds. Of the all-important *Archaeopteryx* we still have only two good specimens from one formation and locality; and we know nothing more of the great race to which it belongs. Of the Cretaceous toothed birds, which have now been known for more than a quarter of a century, the only satisfactory specimens hitherto discovered are a few from one formation in one region of North America.

Again, our knowledge of the history of the elephants has scarcely progressed (except in minute details) since Falconer left the subject at the time when Darwin first referred to it. They can be traced back to a certain point in the Miocene period, where *Dinotherium* seems to be an ancestor of the order in the Old World; but there our genealogy stops. Of *Dinotherium* itself we know very little accurately beyond the teeth; while of its origin and ancestry we can still not recognise a trace among the mammals of earlier date.

Within the last quarter of a century enormous progress has indeed been made in discovering links in the chain of life and in determining the facts of distribution at different periods. The working out of the Tertiary mammals in North America, for example, has opened up a new era in Biology and Geology. But most of the animals discovered and named are known only by a few fragments, which do not reveal even a tolerably complete skeleton. There is very little material for detailed comparison; and only in a few instances is it possible to study individual and local variations. There are very few even of the best known species

of fossil vertebrates which could be described in ample detail, without any assumptions based on the theoretical association of fragments.

Another point worth remembering is this. At the present time all the groups of organisms which are at or near the culmination of their race—are, in fact, dominant types—are represented by numerous genera and almost innumerable species. It is only necessary to think for a moment of such characteristically modern groups as the herring-like fishes, the lizards, the perching birds, and the rats and mice. When, however, we turn to lists of fossils, especially of vertebrate fossils, we note conspicuous poverty in the number of genera and species representing each group even at the period of its maximum development. The reason is not to be sought in the diffidence of palaeontologists to emphasise variations by the multiplication of names: it is solely this, that the geological record preserves only an insignificant proportion of the organisms which have lived even under the most favourable circumstances for burial after death.

A. Smith Woodward.

V

Artificial Formation of a Rudimentary Nervous System

IN my work "L'origine des individus et la construction de l'organisme par les conditions internes"[1] I put forth a mechanical theory of organisation according to which the internal order of beings and their embryological evolution was supposed to be the result of nutritive conditions solely. I admitted the principles regarding inheritance as a consequence of present causes proposed by Delage, and supported his statement with many arguments, but I have of late been induced to consider the whole question from a rather different point of view. There are, in my opinion, no germinative plasma and no mysterious principles in the pronucleus, the composition of which cannot have been modified by the mutilations endured by certain organs of its progenitors. The Monads, the Protists of early geological times, evolved into superior mammals without their having any tendency, marvellous property, germinative plasma or catalytic excitants of glandular origin within themselves.

The internal and external conditions, that is, the agents of progress in the mechanism of nourishment, have doubtless been the efficient causes of an evolution still more astonishing than that of human ovules. Moreover, the study of cellular genealogy, grafts, regeneration, monsters and atavisms obtained by a diminution of nutrition, etc., has demonstrated that there is naught but mechanism more or less obscure and complicated. But the supreme question concerning the origin and functions of the nervous system will be forever a source of impossibilities and embarrassment.

I. There are no essential differences in the vibrations of more or less viscous liquids be they organic or inorganic, for instance: gelatine with glycerine, *Limax* mucus dissolved in acetic acid, albumen of egg, water, mercury or neuroplasm.

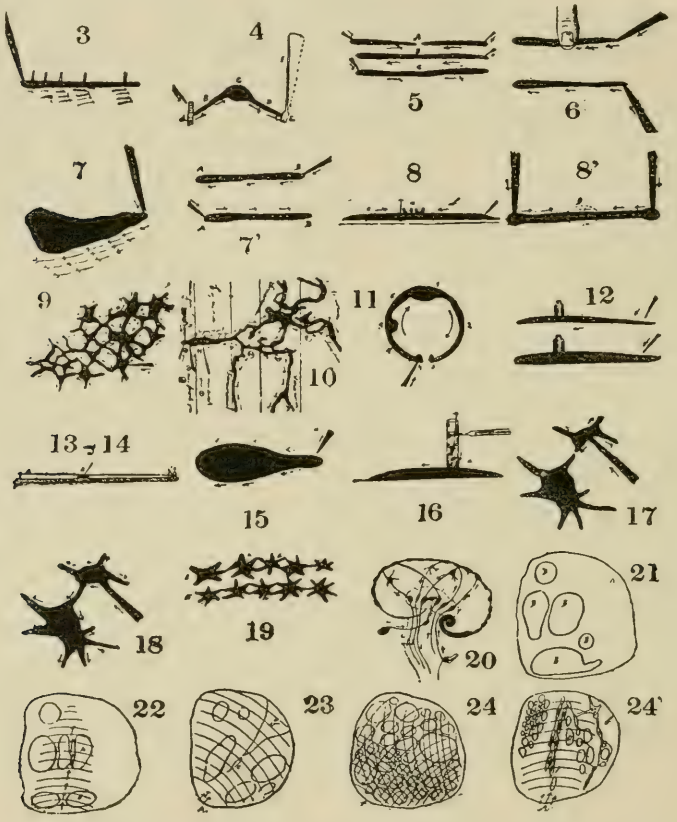
II. The question concerning the origin and functions of the nervous system might be considered as a mechanical problem requiring a laborious solution.

Experiments and comparisons.—Pour in a plate a small quantity of mercury whose fluidity has previously been diminished by the addition of a slight proportion of lead: this will serve the

[1] Sociedad Cientifica "Antonio Alzate." 1898.

purpose of a kind of neuroplasma and receive whatever shape you intend to impress on it, viz., that of a multipolar cell (fig. 17), a cylindrical conductor (fig. 5), etc. Its vibrations may be obtained by the action of azotic or chromic acid, or by means of a small rod or an ant's leg. The vibrations can be verified by fixing a small paper lever on the surface of the liquid, or by receiving a luminous ray reflected on a screen.

Some incidents of nervous transmission are rendered evident by a differential manometer of caoutchouc full of water.



Diagrams illustrating Experiments on Artificial Nerves. For explanation see the text.

(*a*) **Nervous vibration in general.**—1. The rubbing of a nerve of mercury with a soft feather is enough to make it vibrate.

It is, therefore, probable that a very slight mechanical or chemical action (*e.g.* light) would be able to shake the band axis that is, in general, carefully isolated in the middle of a mixture of fats and albuminous matters. The impressionability of liquids is exceedingly delicate. Milne-Edwards [1] has in the experiments per-

[1] Physiologie et Anatomie Comparée, Vol. xii., p. 523 ; Jamin, Cours de physique de l'Ecole Polytechnique. Paris, 1803.

formed by Savart at the College de France seen the shape of some liquid veins change abruptly under the influence of musical notes entirely inappreciable to the ear, that were played at the Luxembourg.

2. The figures below are most interesting :

Velocity of light	.	300,000,000 metres a second.	
„ electricity		180,000,000	„
„ sound	.	331	„
„ liquid waves [1]		10	„
„ nervous vibrations (Lobster)		8	„ (Frédéricq)

" Il ne faut pas confondre la pulsation, l'arrivée d'une onde, avec le mouvement de la circulation lui-même ; on ne peut trop le répéter : *unda non est materia progrediens, sed forma materiae progredientis:* aussi Czermak a prouvé, par des recherches très exactes (sphygmographe à miroir), que tandis que le mouvement du sang diminue de vitesse à mesure qu'on se rapproche des capillaires, la vitesse de propagation de l'onde pulsative va au contraire en augmentant du centre à la périphérie. Onimus a insisté sur ces caractères de l'onde pulsative." (Etudes sur les tracés obtenus par le sphygmographe. *Journal d'anatomie,* 1866.) [2]

It is then extremely probable that a nervous vibration is nothing more than a molecular one, the velocity of which varies in accordance with its conditions, though it never amounts to more than thirty or ninety metres per second. Here are some other proofs:

3. I modified Secchi's classical experiment [3] of pouring a few drops of alcohol on a thin bed of water, by substituting for the former a mixture of alcohol and castor oil, and adding to the water a considerable quantity of linseed oil. Under these conditions a strong agitation takes place. In like manner Longet provoked some local convulsions by touching the motor nerves with alcohol.[4]

4. In most cases mechanical excitation affords real and genuine success,[5] and the excitability of the *Helix* nerves can only be evidenced by means of mechanical and physical irritants. (Milne-Edwards.)

In some animals diverse concretions are present, which oscillate at the least agitation of the external fluid, and increase the mechanic vibration endured by the auditive nervous terminations, rather than translate any currents endowed with a mysterious nature.[6]

I have made a small apparatus tending to demonstrate the influence exercised by otoliths on the vibrations of mercury. It con-

[1] Marey. Mouvements des ondes liquides dans les tubes élastiques. *Journal de physique,* 1875, Vol. ix., p. 257.
[2] Küss et Duval. Cours de physiologie, Paris, 1879, p. 259.
[3] L'unité des forces physiques, Paris, 1874, p. 73.
[4] Traité de physiologie, Paris, 1869, Vol. iii., p. 209.
[5] Béclard. Traité de physiologie, p. 961.
[6] Chatin. Les organes des sens, p. 301.

sists of a drum full of water, having some artificial otoliths prepared after Ray's method[1] (fig. 16).

It is quite surprising that the study of auditive phenomena, or more properly speaking, that of the vibrations of auditive nervous terminations, has not already suggested the explanation of the important facts connected with innervation.

5. Compression abolishes the function of the natural nerve as well as that of the nerve of mercury, but the aforesaid function may be re-established when compression has not disorganised its anatomical elements deeply or divided the thread of mercury (fig. 6). Richet says[2] that an analogy between blood circulation and nervous conductibility might be established: "Quand on applique une pince sur une artère, on interrompt le cours du sang, qui se rétablit dès qu'on enléve la pince."

The facts regarding transmission of electricity, heat, etc., are completely different.

6. If this or that excitation provokes this or that sensibility, it probably takes rise not in the nature of the nerve itself, but in its connections with these or those centres. (Richet.)

7. The wave increases in bulk as a sort of avalanche in the course of its progress through the nerve (Pflüger) and the thread of mercury (fig. 7). The phenomena may easily be observed by fixing several equidistant levers, so that they may rest on the metallic surface lightly (fig. 3).

8. The variations consequent on temperature are probably due to the variations of density of the band axis in the formula

$$v = \sqrt{\frac{e}{d}}$$

for whenever d increases e cools and v diminishes; and may also be due to the duration afforded to the continuance of the muscle's latent excitation (Marey), or to the discharge of carbon dioxide.

9. Richet regarded negative variation of carbon dioxide as a testimony to the nerve's mechanical vibration.[3] They both have nearly the same amount of velocity. Dr R. Jofre, Director of the Laboratory of Medical Electricity, reminds me of similar and considerable modifications of current taking place in microphones, as a sequence to the mechanical vibrations and insignificant stirrings occasioned by an insect's walk for instance.

"La surface inférieure du lobe sensitif de la Dionée attrape-mouches est électro-négative par rapport à la surface supérieure, au moment où la feuille est irritée: au bout d'une demi-seconde, la sur-

[1] Carpenter. The Microscope and its Revelations. London, 1868, p. 775.
[2] Richet. La vibration nerveuse. *Revue Scientifique*, Juillet à Décembre 1882, p. 99.
[3] Milne-Edwards, l.c., Vol. xiii., p. 5.

face supérieure devient à son tour électro-négative et reste ainsi pendant quelque temps."[1]

10. Excitants work either by disorganising the nervous textures or by a subtraction of water, that is, either by mechanical acts of disassociation or else by a modification in the density of the axis-band, or even of the neuroplasma itself. The influence exercised by interstitial water on the excitability of nerves appears to be quite evident, the mere fact of a nerve's desiccation rendering it inexcitable, though it is susceptible of recovering its physical and physiological properties when it has retaken by imbibition the quantity of water necessary for the discharge of its functions. Morphia may perhaps work by modifying the state of hydration of the neuroplasm. It is likewise possible that the blood's circulation and its state of concentration have an indirect influence on the velocity of nervous transmission. (Experimental studies of Mosso.)

11. Most acids work as excitants if applied to nerves. This point being settled, I can further state that a similar result is to be obtained by applying to the point of an artificial nerve of mercury and lead a little chromic or azotic acid, either concentrated or diluted. This also produces: (*a*) Contraction and tumultuous movements. (*b*) Production of waves, by discharge of nitrogen dioxide. This is certainly one of the most weighty demonstrations of my theory.

The neuroplasm, the axis-band in general, ought to vibrate under the influence of acids, because the chemical action practised by the latter on albuminous matters must originate the shocks and vibrations attendant on the subtraction of water, discharge of carbon dioxide, etc.

It is needless to observe that the movements of the artificial nerve of mercury and lead can be explained by the action of gas when the chromic acid is applied. The bioxide of nitrogen slowly issuing from the bottom of some drops of nitric acid (1 in 10 of water), placed on the surface of the metal, begins to whirl round, after the manner of infusoria, or to produce some amoeboid movements that are extremely curious.[2]

12. Physiologists are all of opinion that bile is one of the nerve's excitants.

In certain parts of the nervous system the continual vibrations observed may be due to the excitant action of oxygenated blood, by a discharge of carbon dioxide.

13. Rough excitations have the power of effecting the nerve's vibrations, while slow and gradual excitations are unable, in spite of

[1] *Revue Scientifique*, Juillet à Décembre 1882, p. 735.

[2] Note the amoeboid movements that Bütschli observed in foams and Préaubert in globular rays. "La vie mode de mouvement." Paris, 1898.

their force, to bring the wave forth. (Experiments of Du Bois-Raymond on the destruction of a nerve by a current of increasing intensity.) It is exactly the same with the nerve of mercury: rub it roughly with a piece of feather and this excitation will soon be answered, but it will remain dumb whenever you rub it slowly and gradually with a cloth.

14. There is transmissibility of the excito-motory vibration from a natural or artificial nerve to another when the latter was not in its normal state connected with the former. Their activity can be brought about by means of a stimulus, after every communication between this organ and the centre of innervation has been completely interrupted. (Fig. 5.)

15. It may be objected that the equilibrium of a liquid is not to be altered by such slight vibrations as those that impress the human auditive nerve. Audition would then be impossible. Moreover, the telephone of mercury is established precisely on the principle according to which the transmission of vibrations is effected by means of mercury, and we here repeat that the liquid veins of Savart were modified at the College de France by some music performed at the Luxembourg, that was entirely inappreciable to the ear.

16. The stamens of the *Centaureae* shrink up in their whole length whenever they are submitted to a mechanical excitation, on account of some laws similar to those that rule the contraction of muscles in higher animals.[1]

Electricity affects Sensitives too if discharged in sparks, but it seems to have no influence whatever when it works by continuous currents. It acts in the same manner on nerves.

The sensibility exhibited by the leaves of *Drosera* is such, that an object placed on them and weighing some 000008 gr. merely, is enough to determine their immediate motion.[2]

17. Mr Kühne has succeeded in the construction of a kind of artificial muscle by stuffing a fragment of *Hydrophilus*' intestine[3] with the semi-fluid protoplasm of a certain *Myxomycetes*. This apparatus was affected by electricity as well as if it had been a real muscle. There is not a special force, therefore, but only a liquid capable of vibration.

(*b*) **Muscular vibration.**—I cannot dwell long on the important question of muscular vibration without wandering too far from the principal object of the present paper.

The theory of muscular waves as conceived by Marey and Weber has been fully confirmed by the experiments below:

1. Fix a tube of caoutchouc of small diameter by one of its

[1] Claus, Traité de Zoologie. 1884, p. 14.
[2] Lubbock. La vie des plantes, p. 6.
[3] With nerves and muscles!

extremities and tie it by the other to a weight that can easily slide on a polished surface. At the vibration of the tube, that must be stretched beforehand, the weight is drawn by a quantity equivalent to the extent of the vibration. (Figs. 13 and 14.)

2. By previously stretching a tube it may be induced to vibrate if connected in a point on its middle with a large globule of mercury (nervous termination), the vibrations of which start those of the tube; the locomotion of the weight along the table is the issue of all this. (Figs. 13 and 14.)

This experiment may be considered as a difficult one on account of the computations indispensable to determine the degree of length of the tube and that of the weight which it must attract.

Perhaps the vibration of a muscle is due to the discharges of carbonic oxide.

(*c*) **Acceleration of the pulsatile wave as observed by Landois in elastic tubes previously filled with water.**—This too is easily observed in threads of mercury and serves to elucidate the question concerning certain reflexes that are simply the result of several successive excitations (ejaculation). A. L. HERRERA.

MEXICO, *May* 1*st*, 1898.

(*To be continued.*)

SOME NEW BOOKS

The Backboned Animals

A Classification of Vertebrata, Recent and Extinct. By Hans Gadow. 8vo, pp. xvii + 82. London: A. & C. Black, 1898. Price, 3s. 6d. net.

This is a valuable and convenient handbook for the use of students attending lectures on the vertebrate animals. It can also be used as a notebook, being printed on one side of the paper only.

Dr Gadow defines the Vertebrata as "bilateral symmetrical animals with segmentally arranged mesoderm, with a central solid axis (Chorda dorsalis, extending through the whole length of the body, from head to tail, hence *holochordate*), dorsally of which lies the tubular central nervous system, ventrally the gut; the respiratory organs arise from the anterior portion of the gut." He then proceeds to define the 'sub-phyla' and smaller divisions with commendable brevity and conciseness. As he remarks, there is no reason to enumerate any but the fundamental characters. "For instance, 'the possession of visceral arches, one pair of which is modified into jaws,' is a quite sufficient diagnosis of the Gnathostomata. The presence of an anterior and a posterior pair of limbs is probably quite as essential and peculiar a feature. There are not, and can never have been, paired-limbed vertebrata without visceral-arch jaws; consequently, wherever the converse is the case, we feel certain that the absence of limbs is a secondarily produced feature." Dr Gadow also lays special stress on skeletal characters, not merely on account of their supreme importance, but also because the vast array of extinct animals can only be treated as skeletons. As he well observes, "we do not *know* that the Palaeozoic Fishes did possess an entirely venous heart, nor has it yet been shown that the embryos of Dinosaurs were surrounded by an amnion; but we feel nevertheless certain, because of the laws of correlation which comparative anatomy allows us to deduct from the study of recent creatures. On the other hand, it is quite possible, even most likely, that the Triassic Pseudosuchia had no copulatory organ, and therefore this feature cannot be admitted into the diagnosis of Crocodilia, at least not if they are to comprise the Pseudo-, Para-, and Eusuchia." Finally, the author is to be commended for his selection of generic names. The book is "meant to be used by the present generation," and hence he employs those names under which the whole story of vertebrate anatomy and zoology has been written, ignoring certain recent papers which are rather literary essays than contributions to knowledge.

Dr Gadow recognises two sub-phyla of Vertebrata, namely the

Acrania (represented by the lancelets) and the Craniota (all other known vertebrates). Of the latter there are three super-classes, Cyclostomata, Hypostomata, and Gnathostomata. The Cyclostomata are arranged in the usual manner. The Hypostomata (new term) are the extinct Ostracodermi of Cope. The Gnathostomata comprise the classes Ichthyes, Amphibia, Reptilia, Aves, and Mammalia. The Ichthyes are again subdivided into the sub-classes of Pisces and Dipnoi, and our present knowledge of extinct fishes is specially taken into account in arranging the minor groups, among which 'Ganoidei' survives no longer, except in a footnote. The arrangement of the Amphibia depends chiefly upon Cope, Boulenger, and Zittel. The Reptilia are grouped in eleven sub-classes, of which seven are extinct. The classification of the Aves is based very largely on the author's own researches, while that of the Mammalia corresponds closely with that of Flower and Huxley.

The authorship of the names of the larger divisions is usually mentioned, and the student is helped occasionally by the addition of synonymous terms. The author's researches, however, into the literature of the subject do not appear to have always extended to the original sources, and hence several errors which ought to be corrected in a future edition. Among other terms for which a wrong authorship is given, we may enumerate Antiarcha, Teleostomi, Parciosauri, Dinosauria, and Mesosauri. The equivalent terms, also, are not invariably exact; for instance, the Marsupialia are not precisely the Metatheria of Huxley, but merely the specialised surviving representatives of that sub-class. Moreover, we disapprove of the use of one and the same ordinal term (Lepospondyli) in two distinct classes, and the corresponding wide separation of two such closely related genera as *Keraterpeton* and *Hyloplesion*. The arrangement of the Reptilian orders appears to us very unnatural, the closely-related Crocodilia and Dinosauria being separated by the Chelonia, while the latter again are divorced from the Theromorpha and Plesiosauria, their undoubtedly nearest allies. Recent discoveries in Palaeontology seem to have rendered the Chelonian orders Thecophora and Athecae untenable. We also object to one theory of the quadrate bone being stated dogmatically as a fact in the definition of the Mammalia. For actual errors in the diagnosis, however, we have looked almost in vain. There is nothing more seriously incorrect than the statement that all Cetacean teeth are destitute of enamel, or that *Squalodon* has only one-rooted teeth. The index of proper names, too, is admirably done, most terms having their derivation appended.

At the end of his work Dr Gadow adds a useful chapter on the geographical distribution of the Vertebrata, with a table showing the approximate number of the known recent species. He also gives a fanciful though striking calculation to show how some groups are still in the ascendant while others are distinctly declining. The little volume is, indeed, a welcome addition to the biological student's library, and it deserves the wide circulation which its author's eminence is likely to ensure for it. A. S. W.

Solitary Wasps

The Instincts and Habits of the Solitary Wasps. By George W. Peckham and Elizabeth G. Peckham. 8vo, pp. 245 ; 14 plates (2 coloured). Wisconsin Geological and Natural History Survey Bulletin No. 2. Scientific Series No 1. 1898.

In discussing the problem of Instinct, Darwin wrote, "If it can be shown that instincts do vary ever so little, then I can see no difficulty in natural selection preserving and continually accumulating variations of instinct to any extent that was profitable." This sentence might well serve as the text for the charming book before us, and seems to have been ever present in the minds of our authors during many hours spent in the sweat of the brow, in trying postures, and under a blazing sun in the successful endeavour to learn something from the Solitary Wasps haunting a garden in Wisconsin. The result of their toils may be given in their concluding words :—" The general impression that remains with us as a result of our study of these activities is that their complexity and perfection have been greatly overestimated. We have found them in all stages of development, and are convinced that they have passed through many degrees, from the simple to the complex, by the action of natural selection. Indeed, we find in them beautiful examples of the survival of the fittest." This is a striking contrast to Fabre's remark that had Darwin known the results of his latest observations on the stinging habits of Solitary Wasps, he would have frankly avowed his inability to make instinct enter the mould of his formula. Mr and Mrs Peckham show conclusively that the popular belief that these wasps sting their prey for the purpose of paralysing but not killing, in order that a fresh and not putrid supply of food may be at hand for the offspring is far from correct. Great stress has been laid upon this hitherto accepted belief by Eimer, Romanes, and others, and in view of its wide acceptance among zoologists and the general public it is worth giving a brief outline of the results of our authors' observations on this phenomenon. Out of forty-five species of Solitary Wasps observed by them about one-third kill their prey outright. Of the remainder there is not a single species in which the sting is given with invariable accuracy; in fact, they scarcely sting twice alike since the victims of the same wasp may be killed at once, or may live from one day to six weeks, or even ultimately recover; and this even after treatment by the most skilled surgeons in the hymenopteron world. It is thus at once evident that the sting is not invariably thrust with unfailing accuracy into the nerve centres, and, further, that dead meat is quite as acceptable to the larvae as living flesh—as indeed was fully proved by actual observation. It is of great interest to find that the poison of the wasp's sting has a great paralysing power when introduced into the body of the victim at any point, so that the prey is rendered helpless without the necessity of a complete knowledge of invertebrate anatomy on the part of the wasp. Thus, a leg was broken off a small cray fish, and a *Polistes fusca* made to thrust its sting into the exposed end of the stump, with the result that the cray fish was instantly paralysed and died after a few hours. Similar results followed from causing a *Polistes* to sting a large spider in

various parts of the body remote from the nerve centres, which can therefore only have been affected by the diffusion of the poison.

A pang of regret is almost inevitable as one relegates this well-known zoological fairy-story to the ever increasing category of arm-chair fiction. But any such feelings are more than compensated by the marvellous wealth of observations now put before us. It is difficult to determine on which to dwell in the present notice. We select a few of those bearing on the reputed 'sense of direction' in wasps. It is here shown by numerous instances that these insects do undoubtedly make a careful study of the locality in which they have made their nests, and that a comparatively slight disturbance of the immediate surroundings at once causes them to be at fault. For example, "*Aporus fasciatus* entirely lost her way when we broke off the leaf that covered her nest, but found it without trouble when the missing object was replaced." We might quote many more instances of the same character. We may perhaps be allowed to confirm this opinion by an observation of our own. Some five years ago we chanced upon a nest of *Vespa sylvestris* built in an old tin at the bottom of a ditch; while the wasps were in full work the tin was moved about three yards on to the bank of the ditch; all the wasps that were within the nest at the time of removal noticed as soon as they came to the exit from the nest that their position had been changed, and instead of at once flying off they stood on the edge of the tin for some moments, then took short flights to and fro, gradually increasing their range until they extended to the ditch, the old familiar spot, when they went straight away. It is only necessary to consult the pages of the memoir under notice to be convinced that among wasps, at anyrate, there is no such thing as 'sense of direction,' but that their 'homing' powers are the result of preliminary survey and subsequent memory.

Of the many wonderful devices and signs of intelligence observed, the most astounding is that related of *Ammophila urnaria*. Many individuals of this species were carefully watched making their burrows, catching and stinging their caterpillars, conveying them to the subterranean larder and closing the aperture with lumps of earth, small stones, etc., in order to conceal it from the marauding red ants. As among ourselves so too here some individuals are slovenly and careless in their work, others bestow upon it all the assiduous care of the artist. Of these the last one, already remarkable for her perfect workmanship, reached an excellence that is almost incredible were it not supported by such reliable testimony as that of Mr and Mrs Peckham, and further substantiated by an independent observation by Dr S. W. Williston on another individual elsewhere. This wasp having stored her nest proceeded to fill it up with grains of fine dirt, and "picking up a small pebble in her mandibles used it as a hammer in pounding them down with rapid strokes, thus making the spot as hard and firm as the surrounding surface. Before we could recover from our astonishment at this performance she had dropped her stone and was bringing more earth. We threw ourselves down on the ground that not a motion might be lost, and in a moment we saw her pick up the pebble and again pound the earth into place with it, hammering now here and now there until all was level." Such an

operation as this is nothing less than the intelligent use of a tool, a phenomenon which is well-nigh unexampled even among the higher mammals.

Throughout the whole memoir there occur records of observations of the greatest interest,—for example the part taken by the males of the genus *Trypoxylon* in guarding the nest from parasites during construction; the deterrent effect of the odour of a bug upon a spider; the readiness of some wasps to steal the victims of their sisters, and their unwillingness to accept spiders offered to them by the observers; while hardly less interesting are the not infrequent mentions of apparent failure of instinct. Eight primary instincts are recognised as the result of these very complete observations, viz.—stinging; taking a particular kind of food; method of attacking and capturing prey; method of carrying prey; preparing nest and then capturing prey, or the reverse; mode of taking prey into the nest; general style or locality of nest; spinning or not spinning of a cocoon, and its specific form when made. Variations showing an appreciative adaptation to slight changes of environment are regarded as acts of intelligence.

The work is an excellent example of the value of systematic and painstaking records compiled in the field, and has a healthy outdoor flavour about it. It should be of immense use to many of our Natural History Societies as a model to the zealous amateur of the way in which he may by the careful study of the habits of animals haunting his own paddock or garden contribute to the storehouses of our knowledge facts which help to elucidate some of the most difficult and abstruse problems in bionomics. O. H. L.

Alcoholism

The Temperance Question from a Biological Standpoint. By G. Archdall Reid, M.B. The *Medical Magazine*, June and July 1898. 26 pp.

In the case of a treatise written from a profoundly scientific point of view, fault might be found with the use of the word "Temperance" in the title. As it stands the title suggests a scientific discussion as to moderation in all respects; but the treatise itself is only concerned with moderation in one particular case—in regard to the use of alcohol. So in a scientific discussion the author starts by using a term in its popular sense.

This is not intended as a captious criticism. In a scientific discussion exactitude in terminology is an absolute necessity. Much of the trouble and half the disagreement shewn in such discussions arise from the fact that it is not always easy to understand whether a writer uses a term in its literal or in its popular sense. So that care in this matter is most essential. In a notice in these pages of a previous work by Mr Reid, it was pointed out that he used the term 'evolution' in two senses. He has in the present case profited by the criticism so far as to admit this, and to state in which sense he now employs it.

To come to the question at issue. In regard to Alcoholism Mr Reid puts forward the following as his surmises and arguments:—

1. That acquired characters are not transmissible.

2. That the craving for alcohol is innate in the human race, and is not an acquired character.

3. That human beings differ in the degree in which they possess this craving—that there are the more alcoholically inclined and the less so. But if a man with a great alcoholic craving is able to satisfy this craving with drink, even to his own injury, or is prevented by prohibition measures from satisfying it, his offspring will not in the one case be the more drunken, or in the other case the more sober: they will only inherit the innate craving which he possessed.

4. That the peoples who have had the most experience of alcohol are the least inclined to excessive indulgence therein.

5. That this result has been brought about because alcohol is such a rank poison that it has effectually and constantly killed off the individuals of the nations who were most prone to excessive indulgence, and so left the field free for the breeding of those who were less alcoholically inclined.

It is hoped that this statement does full justice to the position which Mr Reid takes up. It is a very extraordinary position, with a maximum of surmise and a minimum of proof. It suggests a case of a man seeing a flash of lightning and subsequently a house in ruins jumping to the conclusion that the former was the cause of the latter, because such events have been known to occur, without being able to show (1) that the lightning did strike the house; (2) that the house was not in ruins before.

Because the main argument of Mr Reid is that savages have drunk themselves to death. And considering the raw, much adulterated liquid fire with which Christian traders have taken so much pains to supply them, there is not much wonder thereat. But to conclude therefore that the nations of Southern Europe have become temperate because all the alcoholically inclined individuals have been killed off by drink is most rash. What warrant is there for such an assumption in our own case? for, according to Mr Reid, we are in the stage of alcoholism that the southern peoples were in long years ago. But what do we see?—that the most alcoholically inclined, the labourers and artizans, breed in about the proportion of 3 to 1 in regard to the less alcoholically inclined remainder of the population. Wherefore, according to Mr. Reid, we should be getting a more drunken nation with every generation.

This is one of the missing links in Mr Reid's chain of evidence. For in order to prove that alcoholic over-indulgence is an eliminator of the unfit, he must shew that it is so deadly a habit as to kill off its votaries before they have been able to produce as many offspring as the rest of the population. This he does not do. He actually admits that among women alcoholism is not manifest till late in life. But for the purposes of elimination it does not matter in the least if a woman who has passed the procreative period kill herself off in five years with drink, or live to be ninety in soberness. And so with the agricultural labourer,—he may kill himself with drink at forty-five; but if he has at that age, as is not infrequent, added some twelve or fifteen children to the next generation, he has done more to propagate his kind than has the sober professional man who lives to be seventy and has five children.

I may mention in this case a rather instructive lesson learnt from the growing of crops of *Datura tatula* for medicinal purposes. Among the plants there are two marked varieties—(1) The respectable class who most fulfil the purpose for which they are grown, by producing most leaf and stalk per acre ; (2) The waster class, whose sole object in life seems to be to produce seed. Now if the individuals of the two classes were numerically equal in one generation, and seed were saved equally from the whole, class 2 would far outnumber class 1 in the subsequent generation because of its greater fecundity. But class 2 has a worse character than this—it not only produces more seed but it makes sure to ripen that seed the earlier, so that when the crop is cut there is far more ripe seed of class 2 than there is of class 1. Wherefore class 1 should in a few years have almost disappeared to vanishing point.

But artificial selection is far more ruthless than any form of natural selection, so called. We go through the crop with thoroughness to destroy the plants of class 2, so that there may be no seed from them for the next generation. And yet partly perhaps owing to the immensely greater fecundity of the wasters, partly perhaps to an innate tendency of the respectable class to become wasters, in spite of several years' ruthless efforts at extermination, the individuals of the waster class are as numerous as ever. No alcoholic selection would be as vigorous as that. *A fortiori* it would fail even more lamentably.

If Mr Reid could free himself from the fetish of natural selection, and could bring himself to think that acquired characters are inherited, though he may not know how, then he would find himself able to account for the increased sobriety of the nations who have longest known alcohol. Such account would take the following form — that familiarity breeds contempt, and that an acquired character of restraint is inherited until it becomes a second habit performed quite unconsciously.

It is well known that if an individual be introduced to an unaccustomed pleasure he rushes thereat for a while, possibly overdoes it, and then becomes satiated. Instances are all around us. And what is possible for the individual is possible for the race : it may be alcoholically satiated.

As regards the acquired character of restraint, Mr Reid doubts its existence. He puts it in the other way, that the craving is less. Here it is said the craving is the same but the power of restraint is greater. The difference is important. If two cycles of equal weight are started at the top of a hill the tendency to run away is equal in both. But if one be fitted with a brake its running away propensities are checked ; not because the craving to run away is less, but because the brake power—the restraint—is greater. And that is the case with man and alcohol. The sober individual and his parents before him have exercised restraint until it has become a second habit—performed with as little consciousness as walking.

And there is a case strictly analogous, in the acquired restraint which man habitually and often unconsciously uses over his bodily functions. In the case of the urinary organs such restraint is the cause of many diseases and frequently of death, wherefore, according to

Mr Reid, the less restraining portion of the population should be in the majority through elimination, of those practising restraint. But, in spite of any such elimination man, compared to other animals, has advanced enormously in regard to the acquired character of functional restraint.

But Mr Reid says "if acquired characters are transmissible, prohibition is undoubtedly right." That cannot be endorsed; because if there were prohibition there would be no training in the exercise of personal restraint, the power to exercise restraint in necessary cases would be lost, and the last state would be worse than the first.

It is impossible to touch more than the fringe of such a question as alcoholism. But one thing may be said—the best forms of temperance legislation would be—(1) rigidly carried out enactments against food and drink adulteration; (2) nationalization of education.

S. S. Buckman.

Radiation

Radiation. By H. H. Francis Hyndman, with a preface by Prof. Sylvanus P. Thompson. pp. xviii + 307. London: Swan Sonnenschein. 1898.

Radiation links together the principal branches of Physics. It is on this account difficult to obtain a connected view of the subject by a perusal of the ordinary text-books. Mr Hyndman's treatise is, therefore, very welcome. After a brief introduction mainly on wave-motion the author discusses vibrations in matter, in the ether, and lastly, those vibrations such as the Cathode, Röntgen Rays, etc., the nature of which is not yet definitely known. The advanced student will find the book useful, both intrinsically and on account of the copious list of references; whilst it is sufficiently elementary to be intelligible to a larger class of readers with but a slight knowledge of Physics. A few slips have been noticed. The explanation of the sound of organ-pipes and other wind-instruments given on p. 20 is not correct; there is little motion of translation in such instruments, as anyone, who has tried to sound a horn by blowing down it, knows. Again, on p. 130, possibly owing to an error of copying, 'biaxial' has been substituted for 'uniaxial.' It is certain uniaxial crystals which rotate the plane of polarisation when polarised light is transmitted parallel to the optic axis. These small blemishes do not detract from the merits of this interesting book.

The Library of the Dresden Museum

Dr A. B. Meyer has caused to be compiled a Catalog der Handbibliothek des Königlichen Zoologischen und Anthropologisch-Ethnographischen Museums in Dresden, alphabetical and systematic. The volume is an octavo of xxiv., 288 pages, and is issued by the Museum. The entries are brief and recognisable, though not bibliographic, and the serials and publications of academies fall into one alphabet with the authors. The subject indexes should be of much use to readers, especially the ethnographical, which is arranged under countries.

Literature of Russian Geology

We are glad to note that Dr Nikitin's "Russkaya gheologhicheskaya biblioteka" for 1896 [dated 1897] reached England 1st September. Like all records this valuable publication grows in bulk year by year, with, of course, a corresponding difficulty of compilation. This difficulty has now proved insuperable to Dr Nikitin and his collaborator, Miss Marie Tzvetaev, and the work will in future be continued by a special committee of the members of the Commission of the Geological Committee of St Petersburg, of whose publications this biography forms a part. The compiler records 577 papers in the geology of Russia, which, if in Russian, are given a translation of title and brief abstract in French, and if in any other language, are similarly dealt with in Russian.

Bibliography of Scientific Serials

A new edition of Dr Carrington Bolton's invaluable Catalogue of Scientific Periodicals has just been issued by the Smithsonian Institution. Part I. of the alphabetical catalogue is a reprint from the plates of the first edition, with the necessary changes to bring the titles down to date. Part II. contains additions that could not be made to the plates of Part I., together with about 3600 new titles. 8600 periodicals are noted in this catalogue.

Scraps from Serials

The *Proceedings of the Geologists' Association* for August are devoted to a sketch of the geology of the Birmingham district by Professors Lapworth and Watts and Mr W. J. Harrison. The Association, it will be remembered, visited the district this summer, and this modestly-named sketch is 103 pages long, and is fully illustrated by photographs and horizontal sections, and is really a masterly account of the geology of the Midlands. The following list of contents will give some idea of its value :—Physiography; Geology; Archaean rocks of Malvern, Wrekin, Barnt Green, Caldecote, and Charnwood; Cambrian System of Wrekin, Malvern, Nuneaton, and Lower Lickey; Silurian System of Malvern, Abberley, the central area, Lower Lickey, Walsall, Dudley, etc.; Carboniferous System of S. Staffordshire Coal-field, Lower Lickey, E. Warwickshire and Severn Coal-fields; Permian System; Triassic System; Post-Triassic Formations; Petrology; Ancient Glaciers of the Midlands; and last, but not by any means the least important, is a history of discovery in the Birmingham district, a valuable adjunct to most unofficial work. The Geologists' Association may well be proud of the interest shown in them by Professor Lapworth and Watts. A new term 'Charnian' is proposed in the paper (p. 335) by Prof. Watts for the Charnwood series.

No. xvi. of the *Bulletin of the Natural History Society of New Brunswick* contains a portrait and life of Dr James Robb by Dr L. W. Bailey. Dr Robb published as early as 1841 a paper on the geology of New Brunswick in the Reports of the British Association, and followed it up in 1850 with a report on the agricultural capabilities of the province, to which he appended a geological map. He died in

1861. Mr S. W. Kain has a paper on New Brunswick earthquakes. Mr John Moser deals with the mosses, Philip Cox with the batrachia, W. F. Ganong with the natural history and physiography, and G. F. Matthew describes recent discoveries in the San John Group. A bibliography of scientific publications relating to New Brunswick is contributed by S. W. Kain.

No. 30 of the *Journal of the Straits Branch of the Royal Asiatic Society* contains a paper by H. W. Ridley on the birds in the Botanic Gardens, Singapore, one by the same author on plants of the genus *Peliosanthes* of the Malay Peninsula, and yet another on the White Snake of the Salangor Caves, *Coluber taeniurus*. Some valuable papers on Malay Magic, Folk Lore, the game of Chap Ji Ki, and the oldest Malay MS. now extant, make up a substantial and creditable journal.

The Wisconsin Geological and Natural History Survey have started a series of Bulletins. No. 1 is by Filibert Roth, special agent of the United States Department of Agriculture, and is on the forestry conditions of Northern Wisconsin. There is a map. No. 2, by Geo. W. and Elizabeth G. Peckham, is entitled "Instincts and habits of Solitary Wasps," and is a volume of 245 pp., and 14 plates. We hope to refer to these again later.

These Bulletins are to form three series—Scientific, Economic, and Educational. Mr Roth's paper belongs to the second series, and is to be followed by one on the building stones of Wisconsin by E. R. Buckley; the wasp paper belongs to the first series, and is to be followed by Geology of the Pre-Cambrian igneous rocks of the Fox River Valley by S. Weidman; the three first papers in the Educational series will be Collie's Physiography of Southern Wisconsin, Salisbury's Physical Geography and Geology of the dells of the Wisconsin and the Devil's Lake, and Cheney's forest trees of Wisconsin. The Wisconsin Survey was only established in 1897, and is under the direction of Mr. E. A. Birge, Madison, Wis.

The *Boletim de Museu Paraense* for June contains a plan of the Museum of Para and attached Botanical Gardens, with a description by Dr E. Goeldi. Dr J. Huber contributes materials for an Amazonian flora, Dr C. F. Hartt continues his notes on some inedited works of the Geological Commission of Brazil, and Dr Huber writes on *Vochysia Goeldii*, a new species of the Ferrugineae. Photographs of *Hymenaea Courbaril*, L. and *Crudya Parivoa*, De C., are also given.

The *Naturalist* for October contains an account of an ancient Lake-dwelling at Sand-le-Mere, near Withernsea, E. Yorkshire, by Thos. Sheppard. The Rev. W. C. Hey gives a list of Bird names in use at West Ayton, Yorkshire, and there are some interesting notes from the Churchwarden's accounts of Terrington concerning the killing of polecats, which have been extracted by John Wright.

The *Irish Naturalist* for October includes a paper by Dr C. J. Patten, on the birds of Dublin Bay. In *La Feuille des Jeunes Naturalistes* Fournier concludes his paper on the Jura Chain and Eugène Simon concludes his revision of the genera of Hummingbirds. In the *Journal of the Limerick Field Club*, Part II., there is a paper by Dr W. A. Foggerty on the Flora of the Limerick district. As it is full of misprints and includes enough rarities of the Irish flora to cause a pilgrimage, it is but fair to say that Dr Foggerty is

only a compiler, and the authorship of the paper belongs to several botanical members. The *Halifax Naturalist* for October contains a sympathetic life of James Spencer, one of the band of workers associated with the late Jas. W. Davis, Samuel Gibson, and others at Halifax; a continuation of Mr Crump's Flora of Halifax, and other papers.

Further Literature Received

The Principles of Biology, Vol. i., Herbert Spencer: Williams & Norgate. Relations of the Oneonta, Ithaca, and Portage Groups in Central New York; Crustaceans from the Chemung Group of New York; and Sphinctozoan Calcisponge from the Upper Carboniferous of Nebraska: J. M. Clarke, extracts from serials. Mesenteries and Siphonoglyphs in Metridium marginatum: G. H. Parker, *Bull. Mus. Comp. Zool. Harvard. Bol. Mus. Paraense* for June. History of Mankind, Ratzel, Part 29: Macmillan. Life History of Oyster, W. A. Herdman, *Brit. Assoc.* Truth about the Game Laws, J. Connell: Humanitarian League. Zweckmässigkeit und Anpassung, J. W. Sprengel: Fischer. *Bull. Illinois State Lab.*, vol. v., art. 4 and 5. Contributions to the Morphology of the Lepidoptera; Eimer's researches on Eastern Papilios, by K. Jordan: *Novit. Zool.* Catalogue der Handbibliothek des K. Zool. Anth.-Eth. Mus. in Dresden. Beiträge zur Physiologie des Centralnervensystems, Part i., Hypnose der Thiere, Max Verworn: Fischer. *Bull. N. H. Soc. New Brunswick*, No. xvi. Geology of the Basalt Workings, Mt. Rainbow Gold Field; Queensland Minister of Mines. Geological Observations in Ayrshire and other tracts, T. Mellard Reade. Dictionary of Bird Notes, Chas. Louis Hett: Jackson, Brigg. The New Age,—Calcutta, for July. *Journ. Straits Branch, Royal Asiatic Society*, July. The Witness of Science to Linguistic Anarchy, Lady Welby: privately printed. The living Organism, an introduction to the problems of Biology, Alfred Earl: Macmillan. Eclipses of the Moon in India, Robert Sewell, being a continuation of the "Indian Calendar," price 10s. 6d.: Swan Sonnenschein. Report of the Marine Biologist for 1897, Dept. of Agriculture, Cape of Good Hope "P.P., G. 53-'98." Grasses of New South Wales, J. H. Maiden: Minister of Agriculture, N.S.Wales. L'Année biologique, Yves Delage, for 1896: Schleicher Frères. Sea Fisheries, Report on the Trawling Operations of H.M.C.S. "Thetis" under the direction of Frank Farnell, with a scientific report on the fishes by E. R. Waite: New South Wales Government publications, No. 23571. Specializations of the Lepidopterous wing, A. R. Grote: *Proc. Amer. Phil. Soc.*, xxxvii. Vegetation of Lord Howe Island, J. H. Maiden: Proc. Linn. Soc. N.S.W. Flora of Mt. Kosciusko, J. H. Maiden: Misc. Publ. Dept. Agric., N.S.W., No. 241. Tendency of Religion, Col. R. Elias: Chapman & Hall. Lectures on Mathematics, J. C. Lagrange: Open Court Publ. Co., Chicago. Rep. and Trans. South Eastern Union of Scientific Societies, for 1898. Ann. Progress Report of Geological Survey of Western Australia for 1897.

Amer. Geol., Sept.; Amer. Nat., Sept.; Annot. Zool. Japan, vol. ii. pt. 2.; L'Anthropologie, June-Aug.; Bol. Naturalista, No. 8; Botan. Gazette, Sept.; Feuille des jeunes Nat., Oct.; Halifax Nat., Oct.; Irish Nat., Oct.; Journ. School. Geogr., Sept.; Literary Digest, Sept. 10, 17, 24, Oct. 1; Naturae Novit., Sept., and Index to 1897; Naturalist, Oct.; Nature, Sept. 15, 22, 29, Oct. 6, 13, 21; Nature Notes, Oct.; Naturen, Sept.; Photogram, Oct.; Plant World, Sept.; Review of Reviews, Sept.; Revue Mensuelle Bibl. Sci., Aug.; Revue Scient., Sept. 17, 24, Oct. 1, 8, 15; Rivista Ital. Sci. Nat., Sept.-Oct.: Rivista Quin. Psichol., fasc. 8, 9, 10; Scott. Med. Surg. Journ., Oct.; Science, Sept. 9, 16, 23, 30; Scientific Amer., Sept. 10, 17, 24, Oct. 1; Westminster Review, Sept. Oct.

OBITUARIES

GEORGE GREY

BORN 1812. DIED 19TH SEPTEMBER 1898

SIR GEORGE GREY was born at Lisbon, educated at Sandhurst, entered the army in 1829, and became captain in 1855. Retiring from the profession, he conducted two expeditions of discovery in the north-west and west of Australia from 1837 to 1839, the results of his travels appearing in 1841. His collections were worked out by J. E. Gray, J. Gould, and Adam White. In 1841 he was appointed Governor of South Australia, and in 1845 of New Zealand, to which colony the rest of his life was devoted, with the exception of a period of Governorship of Cape Colony. Sir George Grey's valuable Colonial services are too well known to need repetition here, but a few words are necessary to emphasize his services to zoology, which were of no ordinary kind. A deep friendship with Richard Owen led him to seize every available opportunity for collecting the fauna of the lands he visited, and his own inclination led him towards the music, folk lore, and dialects of the native inhabitants. In a letter to Owen in 1849 he deplores the burning of his New Zealand home and the loss of a complete skeleton of moa, three moa skulls, besides numerous other bones, the skeleton of what was probably a *Notornis*, and bones of a quadruped. But with his characteristic courage, he adds, "I will endeavour in the course of this summer to collect again." The *Daily Chronicle* for October 18 has, we are glad to see, started a national memorial to this great public servant.

LOUIS LAURENT GABRIEL DE MORTILLET

BORN 29TH AUGUST 1821. DIED SEPTEMBER 1898

IT is noticeable that in nearly every field of intellectual research some few enthusiastic observers and thinkers are alone the first tillers of the new soil, often amidst troubles and disappointments. So in Anthropology, an important division of Archaeological study, Mr G. de Mortillet, following up the investigations of Mr Boucher de Perthes, was one of the forward workers in this field of research. By his co-operation in compiling and editing the "Matériaux pour l'Histoire positive et philosophique (primitive et naturelle) de l'Homme," together with Trutat, Cartailhac, and others, he greatly aided the advancement of his favourite science, accumulating facts, and forming and distributing useful generalisations as to the probable succession of the various cave-dwellers in Central France and elsewhere. Taking as the basis of his calculations the results of the examination of the caves of Dordogne and neighbouring districts, and the comparison of the animal remains, and the typical stone and bone implements, he sug-

gested that (1) Le Moustier cave is characteristic of the oldest stage of the prehistoric occupancy in this region; then (2) the deposits of Solutré; (3) the Aurignac cave; and (4) that of La Madelaine.

Modifications of this chronological classification have been suggested; but, as planned by Mr G. de Mortillet, it has been useful to the Archaeologists of Western Europe in describing their work and grouping their materials. He followed this system in his steady endeavour to advance the progress of his favourite science by organising Congresses of Prehistoric Anthropology and Archaeology, arranging and superintending the Museum of Antiquities at St Germain (1868), and helping to found the Anthropological School at Paris (1875), of which he became professor.

Among his many writings in the "Matériaux pour l'Histoire," &c., and elsewhere, whether explicit or suggestive, we may refer to his "La signe de la Croix avant le Christianisme" (1866) and "Origine de la Navigation et de la Pêche" (1867): both full of useful information in a clear and carefully ordinate form. His studies of mollusca, the geology of Savoy, the pottery of Allobroges, as well as many contributions on prehistoric peoples and conditions in the periodicals of the day bear witness to his earnest work in his patriotic exposition of the history of those who were the early inhabitants of his beloved France.

He was born in 1821 at Meylan, and educated at Chambery and Paris. He left France in 1849 to escape imprisonment for a socialistic publication, retiring to Savoy and Switzerland, where he arranged the museums of Annecy and Geneva. In 1856 he took scientific work in Italy: in 1864 he returned to Paris, and took up anthropological studies as detailed above.

JAMES HARDY, of Cockburnspath, died in October, aged eighty-four. Dr Hardy was a student of Edinburgh University, and became connected with the Berwickshire Naturalists' Field Club in 1839, in which year he first contributed to the *Proceedings*. He had been secretary for many years to the Club, and as his knowledge was encyclopaedic, north-country zoology and folk-lore have sustained a great loss.

Among others whose deaths have been recently announced are:—Prof. RUDOLPH ADAMY, director of the ethnographical collection at the Hesse State Museum, Darmstadt, on January 14, aged 48; Prof. ANDREAS ARZRUNI, the well-known mineralogist and chemist, in October: JAMES BEHRENS, the lepidopterologist, at San José, Cal., on March 6, aged 74; EUGENIO BETTONI, director of the Brescia Fisheries Station, on August 5, aged 53; Dr ARNOLD GRAF, the morphologist, at Boston, on September 3, aged 30; C. J. H. GRAVENHORST, editor of the *Deutschen Illustrirten Bienenzeitung*, at Wilsnack, on August 24, aged 75; HERBERT LYON JONES, professor of biology at Oberlin College, at Granville, Ohio, August 27, aged 32; Prof. BRONISLAUS KOTULA, the plant-geographer, by an avalanche near Frafoi, on August 19; Dr JOSEPH A. LINTNER, the State entomologist of New York, at Albany, on May 5; DIETRICH NASSE, professor of surgery at Berlin University, at Pontresina, on September 1, aged 38; ROMAN ORIOL, professor of mining at the Academy of Mines, Madrid, and editor of the *Revista Miniera*; JOHNSON PETTIT, the entomologist, at Grimsby, Canada, on February 18; Dr H. PRÖSCHOLDT, the geologist and palaeontologist, formerly of the Realgymnasium of Meiningen, recently, by suicide; Dr GIAMBATTISTA VALENZA, the zoologist, at Pantelleria, on June 15; JOSÉ VILLALONGA, the ironmaster, at Bilbao; Dr JAN DE WINDT, the geologist, drowned in Lake Tanganyika, on August 9, aged 22.

NEWS

The following appointments have recently been made:—C. A. Barber as government botanist at Madras, in the room of the late M. A. Lawson; Dr G. Bode as assistant in the Botanical Institute, Innsbrück University; Dr Arthur Bornträger as director of the Agricultural Station at Palermo; Dr O. Brefeld, of Münster Academy, has been called to the University of Breslau; Miss Agnes M. Claypole as assistant in microscopy, histology, and embryology at Cornell University; E. A. Minchin, of Merton College, Oxford, to succeed Mr Beddard as lecturer on biology at Guy's Hospital; Dr Stephen Crowe and Dr E. S. Pillsbury as assistants in bacteriology at the San Francisco College of Physicians and Surgeons; Dr W. H. Dafert as director of the Agricultural Chemical Station in Vienna; Mr R. A. Daly as instructor in physiography in Harvard University; Dr Vicenzo Diamare as first assistant in comparative anatomy at Naples Universit; S. T. Dunny as secretary to the director of Kew Gardens; Stanley Flower of the King of Siam's Museum at Bangkok, as superintendent of the Cairo Zoological Gardens; W. J. Gies as instructor in physiology at Yale University; Prof. Hofer of Munich as professor of geography in Würzburg University; Wm. Jas. Hornaday of Buffalo as director of the Zoological Gardens in New York, and John Alden Loring of Owego as his assistant; Dr Friedrich Katzer of the Para Museum as geologist to the Sarajevo Museum, Bosnia; Oskar Loew, of Tokio, as chemical plant-physiologist to the Dept. of Agriculture at Washington; Prof. Herbert Osborn, of Iowa Agricultural College, to the chair of zoology at Ohio State University; H. J. Patterson as director of the Maryland Agricultural Experiment Station, *vice* R. H. Miller resigned; A. H. Phillips, as assistant-professor in mineralogy at Princeton University; Dr C. H. Richardson as instructor in geology, and Dr H. S. Jennings (temporarily) in the Dartmouth College, N.S.; Dr Wilhelm Schimper, of Bonn, succeeds Dr Klebs as professor of botany at Basle; Dr Oswald Selliger, of Berlin, as professor of zoology at Rostock University; Dr Mark V. Slingerland, of Cornell, as state entomologist of New York in the place of the late Dr J. A. Lintner; Dr Ernest Stolley, of Kiel, as geologist in the National Museum of Buenos Ayres; Prof. C. H. Townsend as biogeographer and systematic entomologist to the New Mexico Agricultural College and Experiment Station, T. D. A. Cockerell professor of entomology, and E. O. Wooten professor of botany there; Dr C. O. Townsend, of Barnard College, as botanist and plant pathologist for the State of Maryland; Dr Voges, of Berlin, as director of the Bacteriological Institute at Buenos Ayres; Prof. Volkens as one of the custodians of the Botanic Gardens in Berlin; Dr Julius Nikolas Wagner as professor of zoology in the University of St Petersburg; Dr Zukal as professor of forestry at the High School for Agronomy in Vienna.

A *replica* of the Hon. John Collier's portrait of Huxley has been presented to the National Gallery by Mr Collier.

Sir John Murray has resigned the post of scientific member of the Fishery Board for Scotland.

According to *Nature* the vacancy at Kew caused by the appointment of Mr Morris as Commissioner of Agriculture to the West Indies, will not be filled up. It seems a pity that a good botanical post has been lost.

By a slip last month we recorded an item of news that belonged to last year. Mr J. H. Collins was this year the recipient of the Bolitho Gold Medal.

Prof. G. S. Morse has received from the Emperor of Japan the Order of the Third Class of the Rising Sun "in recognition of your signal service while you were in the faculty of science in the Imperial University of Tokio, and also in opening in our country the way for zoological, ethnological, and anthropological science, and in establishing the institutions for the same."

Prof. Dr Simon Schwenderer, Director of the botanical institute of the Berlin University, has been made a knight of the order *Pour le Merite*, in the class of Science and Art.

Professor Knuth of Kiel started in October on a scientific expedition round the world. According to the *Botanisches Centralblatt*, he will be away eight or ten months, and will visit India, Java, China, Japan, Hawaii, and North America.

The Hayden Memorial Geological Award for 1898 goes to Prof. Otto Martin Torell, director of the Geological Survey of Sweden. It is conferred by the Academy of Natural Sciences of Philadelphia, and consists of a bronze medal and the interest on the endowment funds.

Mr W. P. Pycraft of the British Museum has been entrusted with the examination and description of the embryology, pterylography, etc., of the Megapodes and other birds, collected by the Willey Expedition.

Mr W. R. Ogilvie Grant of the British Museum, and Dr H. O. Forbes of the Liverpool Museum, accompanied by a taxidermist, leave on the 28th October for a scientific exploration of the Island of Socotra. They will remain there about three months, and will make a general collection of the natural history of the island. Among other interesting things to be looked for are supposed new forms of a wild goat and a wild ass. Dr Forbes will no doubt get a few lessons in turtle-riding.

We understand that a paper left behind by the late Félix Bernard of Paris, entitled "Recherches ontogéniques et morphologique sur la coquille des Lamellibranches" will be published shortly in the *Annales des Sciences Naturelles*. We are extremely glad to find that some one is looking after the manuscripts of our lamented friend.

The *Botanical Gazette* states that Mr A. A. Heller has resigned his position at the University of Minnesota, to devote himself entirely to collecting. Correspondence having reference to the Exchange Bureau should therefore be addressed to Prof. Conway Macmillan.

Dr Scharff and Mr Welch have, according to the *Irish Naturalist*, made a preliminary dredging trip to Lough Neagh, with interesting results. Dr Scharff and Mr G. H. Carpenter made a preliminary exploration of Macgillicuddy's Reeks in September and hope to publish their results shortly in the above-named journal.

Sir Dyce Duckworth delivered the Harveian Oration at the College of Physicians on October 18. Dr Wm. Ord will deliver the Bradshaw Lecture on November 10. The Goulstonian Lectures for 1899 will be devoted to the pathology of the thyroid gland, and will be delivered by Dr G. R. Murray. The 1899 Lumleian Lectures will be delivered by Dr Samuel Gee. The Croonian Lecturer for 1899 is Prof. Bradbury, and for 1900 Dr F. W. Mott.—*Nature.*

The Biological Station of the University of Indiana will next year be in Winona Park, Warsaw, Ind., eighteen miles from its present station in Vawter Park. One hundred and five students, representing eight States, were present this year, the session closing on August 18. The session consisted of two terms

of five weeks each. Courses were offered in elementary geology, embryology, bacteriology, and botany. According to *Science*, thirteen instructors and assistants attended to the wants of the students.

The Thompson-Yates laboratories of physiology and pathology at University College, Liverpool, were opened on October 8 by Lord Lister.

The $500,000 given to the Medical College of Cornell came from Colonel Oliver H. Payne. This is for a building, his total gift being $1,500,000. Work on the structure has already commenced and it is expected that the building will be finished in 1899. Brown University benefits under the will of Rowland Hazard of Peacedale, Rhode Island, to the amount of $100,000. Mr George A. Gardner has given $20,000 to the Massachusetts Institute of Technology, to be added to the general endowment fund. *Science* also states that Dr D. K. Pearsons of Chicago offers $50,000 to Fair Mount College, Wichita, Kans., provided $150,000 can be raised.

Science states that the Library and Natural History Museum of New Westminster, British Columbia, were totally destroyed by fire on September 11.

The National Museum of Buenos Aires, which has for many years issued substantial contributions to Natural Science in its *Anales*, has just commenced a smaller publication termed the *Comunicaciones* of the Museum, intended for the prompt issue of small and preliminary communications. Except a short note on a new plant (*Prosopanche bonacinai*) by C. Spegazzini, all the papers in the first number are from the pen of the Director, Dr Carlos Berg.

Mr A. S. Woodward, of the British Museum, has this autumn visited some of the Swiss Museums for the purpose of examining fossil fishes. He informs us that, among others, he had the privilege of seeing the original collection left by Agassiz in Neuchâtel. Most of these are British specimens communicated to the author of the "Recherches sur les Poissons Fossiles" by Murchison, Buckland, Lady Gordon Cumming, and other coadjutors in his work. Among them are several type specimens which are commonly supposed to have been lost. Among Stonesfield fossils from Oxford there is the original and almost unique tooth of *Ceratodus phillipsi*, named but not described by Agassiz. The original scute of *Phyllolepis concentricus*, Ag., from the Upper Old Red Sandstone of Perthshire, communicated by Murchison, is also there. Lady Gordon Cumming's contribution from Tynet Burn includes several figured and described specimens. The original supposed jaws named *Plectrodus mirabilis*, from the Ludlow Bone-bed, figured in Murchison's "Siluria," are also in this collection. It is unfortunate that these valuable fossils cannot be transferred to some more appropriate resting-place where they would be properly labelled and appreciated.

The grants for scientific purposes at the British Association of interest to our readers were as follows :—Geology Erratic Blocks, £15 ; Geological Photographs, £10 ; British Carboniferous life-zones, £10 ; Irish Elk in the Isle of Man, £15 ; Prehistoric Flora and Fauna in Canada, £30 ; Drift section at Moel Tryfan, £5 ; Ty Newydd Caves, £40 ; Caves at Uphill, £30 ; Zoological Station, Naples, £100 ; Biological Laboratory, Plymouth, £20 ; Index generum et specierum animalium, £100 ; Migration of Birds, £15 ; Apparatus for keeping aquatic organisms under definite physical conditions, £15 ; Plankton and physical conditions of English Channel, £100 ; Exploration of Socotra, £35 ; Lake Village at Glastonbury, £50 ; Ethnological Survey of Canada, £35 ; 'Anthropological Notes and Queries,' new edition, £40 ; Age of Stone Circles, £20; Physiological Effects of Peptone, £30 ; Electrical Changes accompanying Discharge of Respiratory Centres, £20 ; Influence of Drugs upon the Vascular Nervous System, £10 ; Histological Changes in Nerve Cells, £20 ; Micro-Chemistry of Cells, £40 ; Histology of Suprarenal Cap-

sules, £20; Comparative Histology of Cerebral Cortex, £10; Fertilisation of Phaeophyceae, £20; Assimilation in Plants, £20; Zoological and Botanical Publication, £5. A total amount of £1495 which appears to be the record amount for one year.

The fiftieth anniversary of the American Association for the Advancement of Science was eminently successful; 903 members attended. Dr Alpheus S. Packard read a paper on "A Half-Century of Evolution, with Special Reference to the Effects of Geological Changes on Animal Life," a full report of which appears in *Science* for Sept. 2.

The Report of the Second Triennial Conference of the Irish Field Club Union, which took place July 7 to 13, is published in the *Irish Naturalist* for September. The report is fully illustrated by Mr R. Welch's beautiful photographs, and contains a general account, reports on Arachnida, Hymenoptera, Lepidoptera, Coleoptera, Hemiptera, Mollusca, Botany, and Geology, by specialists in the various groups.

The Natural History Society of New Brunswick increased by thirty-seven members last year, and this according to the Thirty-Sixth Annual Report is a satisfactory state of things. We should like to see the list still higher, for the Society publishes useful and valuable information concerning the province, and maintains a Museum on which it spent 88 dollars in 1897 out of an income of 471. From the *Bulletin* of the Society we learn that the Fredericton Natural History Society, which was founded in 1895, held nine meetings in the winter 1897-8, and through its efforts the schools of the city of Fredericton have been supplied with sets of common minerals for class use, while the High School is being fitted out with a set of minerals of New Brunswick, a very excellent educational effort. We also learn from the same source that Kings County Natural History Society, which was founded in 1897, holds regular meetings the first Saturday in each month, has fifty-two names on its roll, and is divided into five sections—geology and mineralogy, botany, zoology, ornithology, and entomology —much after the style of our own Croydon Microscopical Club and others, and each section is in charge of a committee of three.

From the Annual Report of the Straits Branch of the Royal Asiatic Society for 1897 we note that the income for the year was 1473 dollars, of which 654 were used for the publication of No. 30 of the Journal, while no less than 600 remain as balance. The membership has increased. No. 31 of the Journal is in the printer's hands, and the new map of the Malay Peninsula by Mr van Cuylenburg has been sent to Mr Edward Stanford for publication, and he hopes to have it ready in February.

The British Mycological Society held in September a successful long excursion in Dublin. The *Irish Naturalist* will publish a full report.

The Société de Spéléologie founded in 1895 has fully carried out its programme. *La Feuille des jeunes Naturalistes* points out that the Society has subsidised Mr Sidéridès' work in Peloponesus, that of the Cévenot Club in Les Causses, and the work of Viré, Chevrot, Bidot, Küss, Guérillot, and others in the Jura, as well as some operations undertaken by Mr Voisin, to render accessible the Grotte de Baume-les-Messieurs in the Jura. The Society has published eleven *Bulletins* with a total of 496 pages, and ten *Mémoires*, all fully illustrated, which form a valuable scientific record. There are now 220 members; the subscription is only fifteen francs, and the Society is housed at 7 Rue des Grands-Augustins, Paris.

The Department of Agriculture of the Cape of Good Hope has issued as "G. 53—'98" the "Report of the Marine Biologist for the year 1897" by Mr J. D. F. Gilchrist. In the report for 1896 and in the present report reliable informa-

tion has been published relative to the fishing industry and fishing centres of the Colony. The Colonial Government is now in a position to appreciate the value of this important industry and the possibilities of its development, and to legislate on matter which may arise in regard to it. In order to satisfactory investigate the fishing grounds one of the most modern types of steam vessels was procured, together with a skilled crew, and they set to work with long lines, nets, and trawl. So far it is found that there is within easy reach of Cape Town an excellent trawling ground, rivalling the North Sea in productiveness, and among other excellent fish, soles occur there abundantly, some of them turning the scale at 8 and 9 lbs, from near St Helena Bay. The future work of the "Pieter Faure" as the vessel is called will be the investigation of the Agulhas Bank from Mossel Bay and Port Elizabeth, Knysna, Port Alfred and East London. The scientific aspect of the work will be kept in sight (see *Natural Science*, October, p. 228) but for the present more attention must be given to the industry. Considerable opposition has been made to the operations of the steam trawler, but it has been pointed out that Parliament was only experimenting at present, that proper investigation would be made into the alleged disturbance of spawn, and the fishing limits for ordinary fishermen, but that the store of food available round the coast would certainly be exploited in a country clamouring for cheap food, and that the interests of a large country would outweigh the interests of a few individual fishermen. The report contains some valuable charts, descriptions of a new *Arnoglossus* by Mr Boulenger, and a new genus of gasteropoda *Neptuneopsis gilchristi* by Mr G. B. Sowerby, besides much other statistical information.

The general conference of the International Geodetic Association met at Stuttgart on October 3. Among other matters a programme for a systematic study of variations of latitude, involving the occupation of stations for a term of years, was arranged. Two stations will be in the United States, one in Italy, and one in Japan.

We learn from the *American Geologist* that the International Mining Congress will meet again in 1899 at Milwaukee. The meeting at Salt Lake City in July had an attendance of about 200. One of the chief objects of the Congress is to recommend amendments to the mining laws of the United States.

The Second International Congress of Marine Fisheries was held at Dieppe on September 2, under the presidency of Mr Perrier. There were four sections: (1) For scientific research under Mathias Duval; (2) Apparatus, preparation and transport under Delamare-Debouteville; technical education under J. E. Seigneur; (4) Fishery rules under Mr Roche. Numerous communications were made to the Congress.

The Tenth Congress of Russian naturalists and physicians was held at Kiev on September 3, under the presidency of Mr Bunge. Over 1500 members were present.

The Fifth International Congress of Physiologists will be held at the University of Turin towards the end of September 1901.

The Library of the Millport Marine Biological Station has received a nearly complete set of the "Challenger" publications.

Count Carl Landberg, the Bavarian orientalist, will be the leader of the expedition projected by the Vienna Academy of Sciences, to Arabia. The Swedish steamer 'Gottfried' took the party from Trieste towards the end of October. Prof. Simony goes as botanist, Dr Kossmat as geologist. The chief objects of the expedition are Sabaean inscriptions, pre-Arabic archaeology, and the Mahra language. Dr Layn will go as physician.

We learn from the *Times* that Dr A. G. Nathorst's Swedish Arctic expedition has returned safely to Tromsö. The expedition was most successful, the natural history of King Charles Land is now completely known, and some important hints between the geology of Franz Josef Land and Spitzbergen have been established. Bear Island was surveyed and mapped by Lieut. Kjellstron and Dr Hamberg, as also was King Charles Land, the former on a scale of 1·50,000, and the latter of 1·100,000. Bell Sound was also mapped and the Greenland ice-pack was touched at 78° 1′ N. lat., 4° 9′ W. long. The geology of White Island was ascertained, and the island was found to be covered by an ice-cap from which table bergs are constantly given off. Passing on to Charles XII. Island the expedition visited Freuenberg Bay, Grey Hook, and Danes Island, after which a circumnavigation of Spitzbergen and its surrounding islands was completed. Large collections have been brought back.

We learn from the *Athenaeum* that a new scientific expedition to Central Asia is being furnished by the Imperial Russian Geographical Society in Kasan. The leadership of the expedition is entrusted to Prof. Sorolin, and all the other members of the expedition are professors of the Kasan University. A preparatory sum of 20,000 roubles has been granted towards the cost. The expedition will shortly set out towards Nora, in Central Asia, where the members will pursue geographical, ethnographical, and geological studies.

Also that a Dutch deep-sea expedition, under the conduct of Prof. M. Weber, of Amsterdam, is also to start from Holland during the present autumn. Its range will be less extensive than that of the German deep-sea expedition, as it will be limited to zoology, botany, and oceanography within the eastern part of the East Indian Archipelago.

Mesilla Park has started a science club, under the presidency of Mr C. M. Barber.

Barnard College, U.S.A., will shortly equip a botanical laboratory to be named in memory of Prof. Emily L. Gregory. The Botanical Club have subscribed 500 dollars as a nucleus to the fund.

It is proposed to erect a Biological Station in the Bermudas. Prof. C. L. Bristol of New York University has gone there with a party of students.

According to the *Times* of Oct. 1, a specimen of the 'takahe,' the large rail of New Zealand, *Notornis mantelli*, has recently been found. This bird was first recognised by Owen in 1847 in a collection of bones sent home by Walter Mantell, the types of which are in the British Museum. A second specimen was obtained from Middle Island by some sealers in 1849, and this was also acquired by Mantell. In 1852 a third individual was killed on Secretary Island, the skin of which was preserved. The remains of these two are preserved at the British Museum. No further trace of the bird was seen till 1879, when one was caught alive near Lake Te Anau by a hunter who killed it; it was secured by a Mr Connor, who sold the specimen in London in 1882 by auction for £110, and it is now in Dresden. Fragments of a fifth specimen were found in 1884 also near Lake Te Anau, and these went to Dunedin. The new find makes the sixth recorded specimen of a species evidently rapidly approaching extinction. Another and later letter to the *Times* stated that the writer could furnish as many specimens as wanted.

An interesting balloon ascent was made on Thursday, September 14, by Mr Stanley Spencer and Dr Berson, who reached the altitude of 27,500 feet, only some 1500 less than Coxwell and Glaisher's record of 1862. They descended near Romford after being up some four hours. At 25,000 feet the aeronauts had to breathe compressed oxygen. Numerous observations were made, the results of which are awaited with considerable interest.

CORRESPONDENCE

THE EVOLUTION OF HORNS

Mr J. T. Cunningham's article is too long for detailed criticism in the correspondence columns of *Natural Science*, but perhaps I may be permitted to meet his remarks concerning horns. I take it that the Neo-Darwinian theory of the evolution of horns is as follows. Other things equal, when the hornless ancestors of horned ruminants first began to fight by butting, those individuals best succeeded in the struggle which had skulls most adapted for that mode of combat, *i.e.* those which had the thickest and toughest frontal bones. The next step, after the evolution of strong and solid frontal bones, was the survival of such individuals as had bosses of bone where the impact of the blows most fell. Lastly, the continual survival of those that had the bosses best developed led in time to the evolution of antlers. In the Bovidae a casing of cornified skin was evolved in addition to the bony projections.

Mr Cunningham asks, "Firstly, why do the antlers only begin to develop when the stag becomes mature. Secondly, why are they renewed every summer and drop off in spring." The answer appears to me simple. As he says, "it is at least significant that the males only fight when they begin to breed, and when mature only in the breeding season." It follows, since antlers are used only during the breeding season, that at all other times, being very heavy and cumbersome, they are not only useless, but much worse than useless. They are then causes of elimination only. Natural Selection has, therefore, not only evolved antlers, but has also fixed the times of their appearance. They are not needed by the immature stag, and therefore he has them not. They are not needed by the mature stag after the breeding season and therefore he sheds them, just as in cold climates animals shed their winter coats when the return of spring renders these not only useless but worse than useless. The horns of the Bovidae being much lighter are much less cumbersome than antlers, and for that reason are not shed.

Mr Cunningham attributes the evolution of horns to the stimulation of butting. The primary objection to this is that which applies against all arguments for the transmission of acquired traits, viz., that it is highly improbable that alterations in the soma can affect the germ cell in such a manner that the parental modification is produced in the descendant organism: in other words, it is highly improbable that the modification which butting produces in the frontal bone of the stag can so affect his spermatozoon, situated as it is far distant, that after long separation from the parent organism and union with another germ it develops into an individual which has inborn the special peculiarity the parent acquired. *A priori* the transmission of acquired characters seems impossible, and the onus of proof therefore rests with the upholders of the Lamarckian doctrine. Suffice it to say that the organic world has been ransacked, and no indubitable instance of such transmission has ever yet been proved. Moreover, there is a special objection to Mr Cunningham's theory concerning the evolution of horns, viz. this, that horns do not grow under the stimulation of butting as he seems to imply. Both in the young deer and the adult they complete their growth before the animal begins to fight; during the fighting season they do not increase a grain in weight. If then use, *i.e.* stimulation, does not cause their development in the individual, it cannot of course have caused their evolution in the species.

G. Archdall Reid.
9 Victoria Road, Southsea.

VACCINATION

In the note in September's *Natural Science* on the above subject, it is stated as proved that vaccinia is merely attenuated small-pox. Prof. Crookshank, in his "Bacteriology and Infective Diseases," fourth edition, 1896, in reference to experiments made to prove the identity of the diseases, says: "The results of these experiments have been very generally misinterpreted, and claimed by some as conclusive evidence of the identity of cow-pox and small-pox. Instead of the vesicle being regarded as the most attenuated form of variola, the experimenters are said to have succeeded in producing cow-pox. It is quite true that they produced phenomena indistinguishable from the phenomena of an ordinary vaccination, but that does not mean that they produced the disease cow-pox. The vesicle which followed the inoculation, whether papular or vesicular, was

small-pox. Ceely, Badcock, Voigt, and others, succeeded in engrafting the cow with small-pox, and when suitable lymph and suitable subjects were employed, the virus was so attenuated that a benign vesicle resulted. Similar results were obtained by Sutton and Dimsdale, and identical results by Adams, Guillou and Thiele, by inoculating the human subject with variolous lymph without first engrafting the disease on the cow. Vaccination with variola vaccine is simply a modification of the Suttonian system of small-pox inoculation, only in the first remove the cow is substituted for the human subject." And, in the same connection, Prof. Crookshank adds: "Cow-pox has never been converted into human small-pox, and, in their clinical history and epidemiology, natural cow-pox and human small-pox are so different, that the comparative pathologist is no more prepared to admit their identity than he is prepared to admit the identity of cow-pox and sheep-pox, or small-pox and cattle-plague." Of course these statements are not necessarily conclusive, but they are valuable in showing that, even among those qualified by experience to form an opinion, the identity of the two diseases in question is not regarded by all as proved. U.

Antarctic Exploration

In a special Antarctic number of the *Scottish Geographical Magazine*, received just as we are going to press, Sir John Murray urges the need of a British Antarctic Expedition. The importance of such an expedition has been insisted upon more than once in these columns, and we hope that Sir John Murray's efforts will assist in impressing the mind of the Government. Our maps are a feeble blank concerning Antarctica, and the information we possess as to its fauna and flora is inconspicuous. A few Cetacea, a few seals, and a handful of birds are all that Mr Chumley can record, while as to the Invertebrata, practically all we know was gained in a few dredgings by the "Challenger," during the cruise from the Cape of Good Hope to Australia. Dr Murray's plea is not for a dash to the South Pole, but for a "steady, continuous, laborious, hydrographical and topographical examination of the whole South Polar Area during several successive years" . . . which "would enrich almost every branch of science, and would undoubtedly mark a great advance in the philosophy of terrestrial physics." He asks some of our wealthy citizens to come forward with £100,000, which might be placed in the hands of the President of the Royal Society.

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 29 and 30 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

To the Trade.—Natural Science is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

To our Subscribers and Others.—There are now published Twelve Volumes of Natural Science. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being out of print, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at One Shilling each.

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NATURAL SCIENCE

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NOTES AND COMMENTS

Bubonic Plague in Vienna

The occurrence of fatal cases of plague in connection with the Bacteriological Laboratory at the General Hospital in Vienna has attracted much comment from the press, and is not without its lessons. It may appear surprising to some that such incidents are not of more frequent occurrence. As a matter of fact they are very rare, and this for two reasons. The majority of pathogenic organisms soon lose much of their virulence when cultivated for any length of time outside the body ; some become harmless in a few days, others not for weeks or months, while there are bacteria which seem to retain their pathogenic powers almost indefinitely. In most cases virulence may be restored by suitable passage through the animal body. The chief reason, however, for the rarity of accidents lies in the routine precautions taken in the laboratory when dealing with pathogenic organisms. Such precautions are the first lessons impressed upon the student ; for they are necessary, not only as a safeguard to the experimenter, but in order to preserve the cultures themselves from contamination. Carelessness shows itself at once in impurity arising in the cultures, and although the carelessness which leads to this is not necessarily the same carelessness which contaminates the worker's hands, yet both spring from the same cause, and one is rarely present without the other. Cultivations are carried out on moist media, and micro-organisms growing on such media do not escape into the air and do not seem to constitute any source of danger by inhalation. In the desiccated condition this is not so, but cultures are usually discarded and destroyed before they become dried up, and, moreover, drying is fatal to many kinds of bacteria. In all laboratories the beginner acquires, or ought to acquire, the technique necessary for the protection of himself and his cultures by practice upon harmless organisms. Once acquired, it becomes in time practically a reflex action, and the fear of infection is scarcely present to the mind.

Nevertheless there will always be reckless persons, and accidents will at times occur. Some organisms are especially virulent and dangerous to work with, for instance, the bacillus of glanders. Even

typhoid fever is perhaps at times contracted in the laboratory, and one fatal case of cholera has been definitely traced to this source. Such instances are, however, so rare as to be of historic interest. Laboratory infection is, in fact, a risk almost infinitesimal in comparison with the risks run in the *post-mortem* room or at the bedside, or even in a crowded omnibus. It is the dog in the road who bites you, not the dog you keep chained up in a cage. The recent case of plague at Vienna appears to have been due to carelessness on the part of a drunken laboratory attendant, if, at least, we may trust the accounts which have appeared in the daily press. The lamentable deaths of the physician and nurse in attendance were due to direct case to case infection. The drastic measures which were taken by the authorities appear to have been completely successful in checking the spread of the disease—a gratifying tribute to the efficacy of modern sanitary precautions. The warning against carelessness in dealing with pathogenic organisms is obvious and perhaps timely, but we are far from agreeing with those who, in a moment of panic, would raise an outcry against their study in the laboratory. The good that must result and has resulted from these investigations far outweighs such rare calamities as the recent plague cases in Vienna; the occasional warning should only emphasize the need of prudence and caution in research.

The Borings at Funafuti

Two instalments of news have recently come to hand concerning the coral-boring expeditions, whose plans were related in our July number (vol. xiii. pp. 70, 71). Commander Sturdee's bold experiment of boring in the centre of the lagoon from the bows of his ship (H.M.S. "Porpoise") was a remarkable success. He moored his vessel so taut that it was possible to work the hydraulic boring pipes without risk of their bending or breaking. Mr G. H. Halligan, who was in immediate charge of the boring plant, reports that the first bore reached a depth of 144 feet, the total depth of the bore being 245 feet below the water level of the lagoon, the depth of water to the bottom of the lagoon being 101 feet. The first 80 feet below the bottom of the lagoon passed through sand, composed of segments of *Halimeda* (a seaweed which secretes a jointed stem of lime), and of fragments of shells. This gradually changed into a coral gravel, the fragments being at first small but getting larger at the deeper levels. At the bottom a mass of very hard coral rock was met with, and had to be drilled with a steel chisel. An attempt was then made to enlarge the hole with an under-reamer; but in the process the bore-hole became choked with coral gravel. Efforts to drive the boring-pipes through this with an iron 'monkey' were unsuccessful, and the hole was abandoned. Another

bore was then started 90 feet nearer the centre of the lagoon, in the same depth of water. The time allotted did not permit the bore to reach a greater depth than 113 feet, and the material was similar to that in the other bore.

The deepening of the old bore, discontinued last year at a depth of 698 feet, on the main island of Funafuti, at first proceeded slowly, having been delayed by a temporary failure of water. The party were landed by the London Missionary Society's steamer, "John Williams," on June 20th last. As was anticipated, little difficulty was experienced in re-driving the lining pipes into the old bore, and washing out the sand and rubble which had choked the bore-hole. Pipes were laid from the site of the old bore to some small water-holes from which a supply of fresh water was obtained for the boiler. By July 25th, the re-lining and cleaning of the old bore having been successfully accomplished, boring was resumed, and when the "Porpoise" finally left on September 6, a depth of 987 feet had been reached. The bore last year terminated in soft dolomite limestone at 698 feet, but below this is a hard rock, so hard that the portion of the bore-hole which penetrates it no longer needs to be lined with iron pipes, a condition which facilitates the work of boring.

Mr A. E. Finckh reports that this hard rock is largely composed of corals and shells. At a depth of 840 feet on the ocean face of the reef there is a strongly marked shelf, as shown by the soundings of Captain A. Mostyn Field of H.M.S. "Penguin," and it is considered that this shelf, at the 140 fathoms level, marks the downward limit of the coral formation. The fact that coral rock occurs far below this depth strongly suggests that subsidence must have taken place.

Mr Finckh has conducted successful experiments on the rate of growth of the various reef-forming animals and plants. In conjunction with Mr Halligan he has accomplished some satisfactory exploration of the deep-sea reefs. They have also levelled accurately several lines of section across the atoll, and fixed large and permanent datum-marks for the benefit of future observers. These will show whether the atoll is undergoing elevation or subsidence, and whether it is growing seawards or not. The boring was to continue and Mr Finckh was to go on with his observations until the middle of November. The diamonds, however, had almost run out.

We are indebted for this information to our correspondent, Mr C. Hedley. The results, so far as one can judge, are compatible with Darwin's theory; but we must wait until the cores, already brought to Sydney by Mr Halligan and Foreman Symons have been subjected to thorough microscopic and chemical examination. Until this has been done no opinion can be offered as to the conclusions, but we

can all unite in praising the perseverance of our Sydney friends and the bold conception of Commander Sturdee, and in congratulating them on their truly remarkable success.

The Babel of Terminology

"The Witness of Science to Linguistic Anarchy" is the startling title of a sixty-four page pamphlet compiled by Lady Welby and printed for private circulation. By collecting from different scientific writings a number of passages in which technical terms are used in different senses, or in which such varying use is criticised, she aims at showing: first, that the language employed by scientific authors lacks that very consistency and precision which we have a right to expect; secondly, that concerted action ought to be taken with the view of securing a general consensus in usage. The extracts, chiefly from *Nature*, *Science*, and *Natural Science*, are sorted under the headings: General, Physics, Biology, and [Taxonomic] Nomenclature. These quotations, she believes, to constitute "evidence of an almost incredible state of things in the scientific world."

Incredible though it may appear to "a humble layman," as Lady Welby terms herself, the situation is painfully familiar to every worker in science; and there must be many who agree with us that it is like to go on to the end of the chapter, despite every well-meant effort towards reform. For, what are the causes of this lawlessness? They are mainly two, and those two are very distinct in kind. The first, in the words of a great master of language, is "pure ignorance, madam!" Setting aside the half-educated dabblers in science, how many of us there are who are sometimes impelled just a little out of our depth. *Ne sutor ultra crepidam*, is admirable advice more easily given than followed. This cause is one for which remedies are conceivable. We might, for instance, make publication a penal offence, only permissible to those who had passed the severest examination, and we might burn in the market-place all writings unlicensed by an international censorship which should have absolute control over every new term proposed. We do not say that these remedies are practicable, and we fancy that even Lady Welby will not admit them to be desirable; but the facts of history permit us to imagine them. The second cause is one that lies at the heart of the whole matter, and must persist so long as scientific investigation continues; it is, in fact, the advance of science itself, and for it the only conceivable cure is absolute stagnation, which is no cure at all. The widening of knowledge renders it impossible for a term or a name to have precisely the same connotation to-day as it had twenty years ago. A new species may involve the rediagnosis of its genus; a fresh

observation may necessitate a modified definition. A term that was in use when knowledge was vague and general must be restricted or abandoned as knowledge becomes more detailed and accurate. One man will restrict the meaning of the old word, another will propose a new word; and the number of new terms proposed will vary in proportion to the number of investigators. Each approaches the problem from a different standpoint; and who shall decide which conception is the correct one, which term is the best?

Scientific terminology is, and always must be, in a state of flux. Scientific language is, in this respect, just like any other language. And its evolution is subject to the same laws as govern, not merely all language, but most other mutable things. Whatever those laws may be, we cannot admit with Lady Welby "that in all other directions the first condition of human advance has been the implicit conviction that such advance was possible,—and was imperative." Take Lady Welby's own instance, the alphabet. Which alphabet? In Europe alone there are at least seven alphabets in use, and each of those has many ways of writing its constituent letters. In our own islands the same letters express different sounds according as one is writing Welsh, Gaelic, or English. Even here uniformity is a delusion: in language it is a vain hope. Whether we wish it or no, change will take place, new terms will be proposed, and the fittest will survive.

We would not appear to undervalue the labours of Lady Welby. Those who care to write to her at Denton Manor, Grantham, for a copy of her pamphlet will find it instructive reading. If she will continue her task, and will bring together the passages in which modern scientific terms were originally proposed or subsequently modified, she will render greater service to the cause she has at heart than by the republication of mere querulous criticism. And if towards the second edition that Lady Welby promises we might add a final suggestion, we would write across the blank pages, so thoughtfully provided, the one word, "Index."

The Authorship of Illustrations

In our review of Professor W. W. Watts' "Geology for Beginners" (p. 422) attention is drawn to the figures of fossils said (with some inaccuracy) to have been "prepared for" Zittel's "Grundzüge," and to the fact that no reference is made to the original source of those figures, many of which are certainly not to be ascribed to Professor Von Zittel himself, though the student might be led to suppose that they were. Such are Walcott's restoration of a section across *Calymene senaria*, here wrongly assigned to *Triarthrus becki*; Holm's restoration of *Olenellus kjerulfi*; Owen's picture of the pearly nautilus

in its bisected shell; Henry Woodward's restoration of *Pterygotus anglicus*. We think it right to remark specially on this feature, because it is an instance of a habit far too common with writers of text-books. Let us look at a few other recent cases. Professor W. B. Scott, in his "Introduction to Geology," favourably noticed by us last April, has copied Dr Traquair's first (1888) restoration of *Pterichthys* from Bashford Dean's "Fishes, Recent and Fossil," and has marked it "From Dean after Smith Woodward." The same legend is attached to Dean's own faulty attempt at a restoration of *Dipterus*, and we can only hope that Smith Woodward feels complimented. Parker & Haswell's "Text-book of Zoology" teems with similar errors. Ray Lankester's figures of the head shield of *Cephalaspis* and Traquair's restorations of *Pterichthys* are marked "From the Brit. Mus. Cat. of Fossil Fishes," although the writer of that Catalogue was careful in both cases to acknowledge the authorship of the figures. Similarly Traquair's restorations of *Palaeoniscus macropomus* and *Platysomus striatus* are marked "From Nicholson and Lydekker," in spite of those authors having expressly noted the figures as "after Traquair." So, too, a restored outline of the bones of the shoulder-girdle of *Plesiosaurus* is said to be "after Zittel," who all in vain had taken care to state that it was "nach Owen." Then comes Mr Beddard with his book on birds, reviewed by us this month, and assigns to Andrews the authorship of Ameghino's figures of the skull and pelvis of *Phororhacos inflatus*, simply because Andrews copied those figures (with due acknowledgment) in his paper in *The Ibis*. Examples crowd to our hand, especially in books by lesser writers, but we have only room for one more scapegoat. Last year Professor A. Issel published a "Compendio di Geologia." In this Huxley's old (1862) restoration of *Holoptychius* is attributed to "Traquair," while Pander's ancient restoration of *Asterolepis ornata* is marked "Pterichthys (Traquair)." As this was published by Pander in 1858, we should imagine that Dr Traquair was then a schoolboy.

Now is not this a parlous state of affairs, evincing what the costermonger described as "a very careless handling of the truth" by a class of men whose studies are supposed to lead them to a special reverence for truth and accuracy? To depict an elaborate dissection or to construct a restored figure of an extinct animal, is just as much an embodiment of the results of original research as pages of written description. The attribution of such figures to the last text-book writer that has happened to copy them is an act of scandalous injustice to those to whose patient research the said figures are due. The purchase of *clichés* or of permission to photograph woodcuts is a purely commercial affair, and it matters not at all from whose work a restoration was last copied. In any case, if

an advertisement has to be given, it need not be in the form of an untruth. The student wishes, we presume, to be directed to the original source of the figure and not to be sent on a wild-goose-chase from text-book to text-book. On the latter principle the whole of "Hamlet" might be attributed to writers in the daily press.

We can put the point quite clearly to the gentlemen referred to above. How would they like to see the results of their own original researches placed to the credit of any author that chanced to quote them? Or how would they like to have the obsolete ideas of older authors fathered on them? At the least they would denounce such action as a gross injustice, and they might say that it contravened the most elementary notions of morality.

Photography in National Museums

We have so often urged the advantage of having a permanent photographic establishment connected with our museums, that we welcome the address recently delivered to the Royal Photographic Society by its President. All the more do we welcome it, seeing that the President is the Earl of Crawford, who, as one of the Trustees of the British Museum, speaks with knowledge of the need and of the practicability of supplying it without any additional cost to the nation. Berlin and Munich, he pointed out, have shown that a photographic department can issue the finest work, can be of incalculable value to institutions in all parts of the world, and yet can be self-supporting. He pleaded forcibly that some such establishment should be attached to the British Museum.

In this connection it is satisfactory to learn that at least one small step in the desired direction is being taken at the Natural History Museum. A studio for the use of photographers is being built, and when this is completed it will be possible for investigators to photograph the specimens in the collection under better conditions of lighting and of quiet than are now attainable. We do not, however, learn that it is contemplated to attach a photographer to the establishment, or even to train an attendant as operator. The chief use to which the studio will be put will be the photography of specimens for the illustration of the Museum catalogues. Those who wish to utilise the accommodation for other scientific purposes will, whether they be connected with the Museum or no, still have to import their own apparatus and their own photographer at an expense which weighs heavily upon most students of natural history, and seriously hinders the proper illustration of scientific papers. However, the wedge is being slowly driven in; we must be grateful, and we live in hope.

The Frank Buckland Collection

Among other matters that cannot but prove interesting reading to the authorities at South Kensington Museum, the Report of the Select Committee recently issued contains the following paragraph:—

"We recommend that the Museum of Fish Culture should be abolished. Previous recommendations to this effect have been made. The Secretary and the Director both agree that it should be removed, and it has already been offered to two public bodies, being rejected by both. The fact is that this collection is dangerous owing to the large amount of alcohol in which the fish are stored; it is obsolete, not having been revised or increased for several years; and it does not carry out its obligations under the testamentary conditions of Professor Buckland's will. It occupies a good deal of space. Opinion being unanimous, we hope that this collection may disappear without delay."

The state of things is no doubt disgraceful, but the remedy proposed seemed too severe to many naturalists, and among others to the Piscatorial Society, which appointed a committee, consisting of Dr C. S. Patterson, Mr G. J. Chatterton, and Mr C. E. Walker, to investigate the matter. The following report was unanimously adopted by the Society:—

"The committee inspected the collection, which they found in a deplorable condition, and quite inadequate to carry out the testator's intentions, evidently owing to absolute neglect since it was taken over. There being no catalogue it is impossible to determine how much of the original collection still exists. The purchased additions apparently consist of something less than two dozen specimens, the majority of which have no direct bearing upon British fish industries. A large amount of the space allotted to the exhibit is taken up by objects which, however interesting in themselves, have no connection with either fish or fisheries. Your committee fully endorse the opinion of the Select Committee of the House of Commons as to the danger arising from the specimens preserved in spirits, as the building is entirely unsuited for the storage of such exhibits, but fail to see the point of the objection as regards the Buckland bequest, inasmuch as the majority of the fish in alcohol belong to the Day collection, which is not in any way an industrial exhibit, and should be placed in the Natural History Museum. As regards the testator's intention to provide a consulting and reference room for his fellow-countrymen, whether interested in sea or river fisheries, your committee are of opinion that such an educational centre is urgently needed, and that the collection in question, although inadequate through neglect, is capable of being brought up to date and of taking the place contemplated for it by the donor. Subject to Mrs Buckland's life interest a sum of £5000 was bequeathed to the Director and Assistant-Director of the South Kensington Museum in trust for the British nation to provide lectures on fish culture in connection with this unique series of specimens. Your committee, however, have

failed to ascertain what has been done with this money. All that they know is that no such lectureship exists, despite the statement of Mr George Bompas in his 'Life of Frank Buckland,' published in 1885, that after the death of Mrs Buckland '£5000 was given to found a lectureship.' Your committee have read with regret the recommendation of the Select Committee . . . [quoted above].

"In face of this recommendation the matter is urgent, and your committee are of opinion that steps should be taken at once to avert such a calamity as the extinction of this collection would prove to be. With this object they advise that the whole of the facts of the case be made public through the instrumentality of the Press, and, if necessary, by the question being raised in the House of Commons."

We agree that the destruction of the collection and the ignoring of Frank Buckland's intentions would be matter for great regret. Could not the Marine Biological Association take the matter up?

New Museum Buildings at Liverpool

On July 1, Sir William Bower Forwood, Chairman of the Library, Museum, and Arts Committee of the City Council, laid the foundation stone of the new Technical School and the extension of the Museum Buildings at Liverpool. The present museum buildings stand on a plateau sloping abruptly towards the west. By excavating the slope, which consists of Permian rock, down to the level of Byrom Street, sufficient accommodation, three storeys in height, will be provided for the Technical Schools, while the Museum galleries will be carried forward on their present level over the Schools. The architect of the new building is Mr E. W. Mountford of London. The extension of the Museum will be 90 feet above the level of Byrom Street, and will measure from N. to S. 162 feet, and from E. to W. 190 feet. The galleries, of horseshoe shape, will be continuous with those in the existing building, and will not be divided in any part of their course by walls or partitions. They will be 420 feet long and 33 feet wide; the lower, to contain the invertebrates, will be 19 feet high and lighted from the side; the upper, to contain the vertebrates, will be 27 feet high and lighted from the roof. There will be new and well-appointed laboratories for the Director and his assistants, as well as new administrative offices. The buildings will be of brick, faced with Stancliffe stone from Darley Dale, Derbyshire. They will be ventilated and heated by 4 miles of 3 in. pipes, discharging into every room purified and warmed air to the amount of eight million cubic feet per hour. The stairs are to be of stone, the floors of concrete, and the roof chiefly of steel. These buildings will be, next to St George's Hall, the largest in the city and probably the finest museum buildings in the United Kingdom outside London. It is

most cheering to see the attention now being paid to scientific and technical education and to intellectual culture in the great commercial city of Liverpool.

The Reduction of the Teeth among Mammals

The professors and lecturers in our Agricultural and Veterinary Colleges have many opportunities of making substantial contributions to biology. They deal with a series of problems which the student of organisms uninfluenced by artificial surroundings can rarely hope to find within his range. They observe changes in the structure and function of organs which they are able to correlate with known factors in the process of domestication or cultivation of the animals or plants they happen to study. They also have facilities for embryological research among the vertebrates, far exceeding those obtainable under any other circumstances. We who are interested in the purely theoretical questions of biology thus welcome with peculiar gratification any contribution from a naturalist who has taken full advantage of these favourable conditions, and enlivened the dull routine of teaching with comparatively broad generalisations.

One such contribution is contained in the seventy-ninth annual *Programm* of the Royal Wurtemberg Agricultural Academy at Hohenheim, dated 1897, but received from the author, Professor W. Branco, a few weeks ago. Professor Branco is the distinguished palaeontologist who succeeded Quenstedt in the University of Tübingen, but was unfortunately compelled by ill-health to relinquish the duties of the professorship after too brief service. Having now happily recovered, he devotes his energies to the Academy of Hohenheim, and his unusually wide sympathies in biology still stand him in good stead. A short time ago he published a description of some peculiar teeth, almost human in shape, from the Upper Eocene or Lower Miocene 'Bohnerz' of Wurtemberg (*Jahresh. Vereins für vaterl. Naturk. Württemb.*, 1898, pp. 1-140, pls. i.-iii.). He now follows this memoir by the present contribution, which discusses the nature and origin of the reduction of the dentition among mammals in general.

After some preliminary considerations and a broad outline of the facts, Prof. Branco applies the knowledge he has obtained in the course of his professorial duties. He points out that one principal cause of the reduction of the teeth is the shortening of the jaws. Among domesticated animals this shortening is shown to be due to at least two causes. It happens when the food requires comparatively little mastication; animals of any particular race fed upon soft food produce short-faced descendants, while others of the same race continually fed upon hard food always retain longer jaws and

face, also more slender limbs. Continual in-breeding tends to keep the jaws and face elongated and the limbs slender; while the frequent accession of new blood has precisely the reverse effect. In connection with this Prof. Branco ingeniously remarks, that in early Tertiary times, when there were much fewer mammals than in later times, in-breeding must have been comparatively common and may thus account for the universal long jaws and numerous teeth characteristic of all genera of the period.

A second cause of the reduction of the mammalian dentition is the preponderating growth of one or more of its components; such as the excessive development of the canines in *Sus*, and of the last molar in *Phacochoerus*.

As already long recognised, teeth also disappear when their function is lost. Hence the loss of the upper incisors in ruminants and most of the incisors in elephants, when the tongue and the trunk respectively usurp their functions. Hence also the loss of canines when effective weapons in the form of horns appear.

Finally, it follows from these considerations that changes in the mode of life and feeding have always been most potent factors, not only in modifying the individual teeth, but also in tending towards their reduction in number and the preponderating development of a few.

A New Peripatus

A good deal was said about the Peripatidae in the pages of *Natural Science* in connection with the publication of the fifth volume of the Cambridge Natural History, and in the series of short articles giving the opinions of various experts on the classification and constitution of the Arthropoda.[1] Since that time the literature of this interesting family has been enriched by several papers, by far the most important of which is Dr Willey's memoir on the species he procured in New Britain (see *Natural Science*, vol. xiii. p. 280). New Britain is an island lying off the east coast of Papua and forming part of the Austro-Malaysian sub-region of the Australian Region of Sclater and Wallace. Hitherto no member of this group had been discovered in this area, although several species have long been known from the adjacent sub-regions of Australia and New Zealand. One species too has been recorded from Sumatra; but some authorities, including Dr Willey, seriously doubt the accuracy of this locality on the grounds that this alleged Sumatran species is apparently generically identical with the Neotropical members of the family. Other zoologists, on the contrary, not unmindful of such facts as the distribution of the existing species of tapirs in the large Malay islands and in tropical America, are not quite so sceptical and see

[1] Vol. viii. pp. 122, 215, and 285; vol. x. pp. 97 and 264.

nothing particularly unusual in the existence of close relationship between a Sumatran and Brazilian species. Now if Dr Willey's *Peripatus* had proved to belong to the same type as the Sumatran and American species, then presumptive evidence in favour of the correctness of the locality assigned to *P. sumatranus*, would have been supplied. This, however, is not the case. Nor is it a particularly surprising fact, considering the great faunistic differences that obtain between Sumatra and New Britain with regard to many groups of animals, especially those, like the Peripatidae, with very limited means of dispersal. More surprising is it on zoogeographical grounds that the New Britain species also presents no near affinity with the species that are met with in Australia and New Zealand, seeing that the latter are congeneric. So, too, is it equally distinct from the last remaining type, namely, that which inhabits S. Africa. The great interest attaching to this species lies in fact in the circumstance that it occupies an isolated position and is distinguishable from the rest of its allies in exactly the same way that they are distinguishable from each other, that is to say in external structural characters, in details of internal anatomy and in the mode of development of the embryo. And since the other previously known types had been designated by generic names, there was no other course open to Dr Willey than to assign a name to his new species. Unfortunately, he prefers, for unstated, but no doubt excellent reasons so far as they go, to regard the sections of Peripatidae as merely of sub-generic importance—unfortunately, because in nine cases out of ten, such titles always assume the higher rank, and no doubt *Paraperipatus* will follow its destiny. The result is that this new form rejoices in the sixteen-syllabled title of *Peripatus* (*Paraperipatus*) *novaebritanniae*. Regarding these divisions for the moment as genera, we now have the following: *Peripatus*, Neotropical Region and Sumatra; *Peripatopsis*, S. Africa; *Peripatoides*, Australia and New Zealand; and *Paraperipatus*, New Britain. The characters in which these genera resemble and differ from each other are usefully summarised in tabular form on p. 37 of Dr Willey's memoir.

A New Palaeozoic Sponge

Dr J. M. Clarke of Albany has sent us a paper contributed by him to the *American Geologist* (vol. xx. pp. 387-392, pl. xxiii., Dec. 1897), on "A Sphinctozoan Calcisponge from the Upper Carboniferous of Eastern Nebraska." The new sponge, to which Dr Clarke has given the name *Amblysiphonella prosseri*, is nearly cylindrical in form and about 100 mm. (4 inches) in length. It is built up of a vertical series of chambers or segments, and a central cloacal tube extends throughout its length. The interior wall, the transverse septa roofing the chambers, and the cloacal walls, are

directly traversed by canals. The chambers are also irregularly divided by thin, apparently non-perforate, partitions. The walls are now of crystalline calcite of secondary origin and nothing is known of their original characters.

This sponge is closely allied to forms from the *Productus* limestone of the Salt Range, India, and from the province of Asturias, Spain. So far as mode of growth is concerned, these Carboniferous Calcisponges resemble the similarly segmented sponge genera *Barroisia*, *Thalamopora*, *Tremacystia*, &c., from the Lower and Upper Greensand, but no true comparison is possible until the minute nature of the wall in the palaeozoic forms has been ascertained. If this should prove to be spicular in character, there can hardly be any further doubt of their relationship to the Cretaceous genera referred to. These latter have recently been placed with the Sycons by Dr Rauff, but their true systematic position is not yet fully established. It should be remembered that the segmented mode of growth is not limited to the Pharetron Calcisponges; it is also well shown in the Jurassic Hexactinellid genus, *Casearia* of Quenstedt.

Botany and Agriculture

We have received several publications which are of interest to the agriculturist abroad. From New South Wales comes a "Manual of the Grasses" of the colony, issued by the Government botanist, Mr J. H. Maiden. It is a praiseworthy attempt to bring the native grasses before the farmer and botanist, not only for purposes of identification, but as a guide for their cultivation and improvement. New South Wales has, according to present knowledge, about 200 indigenous species included in 56 genera, and of each of these the author gives an adequate botanical description (in English), with references to figures previously published, and in addition any information he has been able to gather on the value as fodder and other uses. There are also notes on habitat and geographical range in the Continent. Of some of the species useful figures, including habit and floral dissections, are given.

Mr Maiden also sends a pamphlet on the vegetation of Lord Howe Island, to which he paid a short visit at the beginning of the year. His collections have resulted in several additions to the flora of this little island, which, as at present known, includes 217 indigenous species of flowering plants and ferns, with 20 introduced ones. The chief difficulty with which the farmer has to contend is the wind; it is said to be no uncommon thing for it to blow strongly for three months at a stretch. The wind-break question is therefore an important one, and every patch of cultivation is protected by belts of indigenous or planted trees.

A contribution towards a Flora of Mount Kosciusko, also by Mr Maiden, is an account of an expedition of a few days to the highest mountain in Australia, 7328 feet or more above sea-level. It includes a list of the plants found at different altitudes.

The U.S. Department of Agriculture sends a couple of pamphlets issued by Dr Hart Merriam, chief of the Biological Survey. One, entitled "Life Zones and Crop Zones of the United States," is invaluable to the farmer. It embodies the results of ten years' study of geographical distribution applied to practical agriculture. North America is divisible into seven transcontinental belts or life-zones and a much larger number of minor areas, each of which, up to the northern limit of profitable agriculture, are adapted to the needs of particular kinds or varieties of cultivated crops. A coloured map shows the distribution and limits of the zones, while the text contains lists of the varieties of cereals, fruits and other crops, which may be profitably grown in each area. Dr Merriam expresses the hope that his report will serve to emphasize the extreme wastefulness of indiscriminate experimentation, by which hundreds of thousands of dollars are thrown away each year in futile attempts to make crops grow in areas totally unfitted for their cultivation. It also suggests alternatives where, owing to increased competition or diminished demands, the farmer receives an inadequate return for his labour. For instance, in northern New York and elsewhere, where dairying is an almost exclusive but unprofitable industry, the land is shown to be adapted for sugar-beet, or several excellent varieties of wheat and other crops to which little or no attention is now given. The second bulletin, by Prof. C. S. Plumb, is a special instance on the same lines. It deals with the geographic distribution of cereals, and indicates what varieties may be grown with profit in each area.

The Rhone Beavers

These unfortunate animals decrease each year by reason of chase and flood. Mr Galien Mingaud writes in the *Revue Scientifique* that no less than nine of these rodents were captured during 1897 in the delta of the Camargue, at the junction of the Rhone with the Gardon. The beaver goes up this latter river as far as Pont-du-Gard. Two years ago Mr Mingaud addressed a note to public officers and to naturalists at large, asking for the protection and preservation of this animal. He proposed that the riparian owners should turn their attention to 'castoriculture' as a source of revenue, and thus enrich themselves while preserving the stock. Fortunately the old prize of 15 francs a head for all beavers killed, which was promoted in 1855, was suppressed in 1891 on the urgent representations of Mr Valéry Mayet. Since 1890, Mr Mingaud has kept a

record of all deaths which came to his knowledge, and he finds it average 8 to 10 a year. As he points out, Washington has established a colony of beavers in the National Park, and it is a complete success. The animals are kept to a woody valley through which runs a small water-course, and they there construct their dams and tunnels, quite familiarised to the occasioned presence of man, who can watch their daily life and works unheeded. He hopes that France will similarly protect the few last beavers remaining in the Camargue delta, and we cordially echo his sentiments.

A. T. Masterman 'on the Diplochorda'

Some preliminary notes by Mr Masterman in the *Proceedings of the Royal Society of Edinburgh*, for 1896, and also in the *Zoologischer Anzeiger*, paved the way for a more detailed paper of great interest in the *Quarterly Journal for Microscopical Science* (vol. xl., 1898).

The paper is divided into two parts—(1) on the structure of *Actinotrocha*, and (2) on that of *Cephalodiscus*. As the result of an exhaustive study of *Actinotrocha* (the curious larval form of *Phoronis*) by means of sections, Mr Masterman comes to the conclusion that the close similarity in structure to the three members of the group now commonly called Hemichorda points to a genetic connection, but that *Phoronis* and *Cephalodiscus* should be considered as constituting a distinct sub-division of the Chordata, for which the name of Diplochorda is proposed, owing to the possession by its members of paired lateral notochords. *Balanoglossus* is supposed to represent a later phylogenetic stage, in which these lateral notochords have fused in the median line. As there are certain animals which have always been objects of disagreement on account of their generalized types and the doubtful nature of their genetic relationships, so, too, there have always been certain organs or structures around which controversy has continually raged; the notochord is one of these, as students of the literature of *Balanoglossus* are well aware. If Mr Masterman's views are correct, and the paired structures he describes both in *Actinotrocha* and in *Cephalodiscus* are really of notochordal value, he has established a fact of the greatest scientific interest and phylogenetic importance, and we cannot but think he has made out a very good case for the homology. The figures on plates 25 and 26 are, we think, especially instructive, but they are nevertheless not universally regarded as convincing. Mr S. F. Harmer, whose views on the notochord of *Cephalodiscus* have been expressed in the *Zoologischer Anzeiger* (vol. xx., p. 342), while fully agreeing with Mr Masterman as to the relation of *Phoronis* with *Balanoglossus*, does not admit the homologies with *Cephalodiscus*, on which the main argument depends. Mr Masterman claims to show that the structure described by Mr Harmer as the notochord in *Cephalodiscus* is really the subneural gland, and

that lateral notochords, similar to those in *Actinotrocha*, are found in addition. The subneural gland, according to our author, is the same structure as that in Ascidians, and probably corresponds to the hypophysis in Vertebrata and the proboscis-vesicle ('Herzblase' of Spengel) in *Balanoglossus*. Mr Harmer puts the contrary view very fairly and clearly, but he is evidently at a loss to explain the alleged lateral notochords, if they are not what our author believes them to be. We hope that Mr Masterman will be able, in the absence of ontogenetic proof of origin (for *Cephalodiscus*), to bring more exact histological evidence to bear upon this vexed question, and to prove his point more completely.

Meanwhile, he has already done much in convincing us of the relationship between *Phoronis* and *Balanoglossus*, by comparison with *Actinotrocha*, and we are encouraged to hope for further light upon those matters which remain doubtful.

Change of Address

Once again *Natural Science* changes its address, and this time, not the address only, but the editorial staff. The reasons for this were given at length in our October number. They need not be repeated here; but we cannot omit to thank the numerous friends who have, since then, extended to us sympathy, both publicly and privately. We trust that all our subscribers, readers, and contributors will continue to support *Natural Science*, which, we have every reason to believe, will continue to deserve their support. There will be no change in the policy of the Review, no break in continuity, and no lowering of the standard hitherto set before it. But those who wish well to the future of this journal, should remember that it lies with them to see that it has a future. Editors cannot edit unless there are contributions of articles, notes, and news; publishers cannot publish if every reader reads the copy of a friend or of a library. Send at once your contributions and your subscriptions for Volume XIV. to the new editorial and publishing offices. The address is—Mr Young J. Pentland, 11 Teviot Place, Edinburgh.

And now the type-writer ceases for a moment to click, but the hands remain on the key-board. The days may have been anxious and the nights weary, but the work has brought us many interests and many friends, and it is hard to withdraw from it. Yet the last word must be written, and we write it with its oldest and fullest meaning—"Good-bye."

I

Mr Herbert Spencer's Biology [1]

WHAT strikes one most forcibly on reading the "Principles of Biology" in this new and enlarged edition is the extraordinary range and grasp of its author, the piercing keenness of his eye for essentials, his fertility in invention and the bold sweep of his logical method. In these days of increasingly straitened specialism it is well that we should feel the influence of a thinker whose powers of generalization have seldom been equalled and perhaps never surpassed. In no narrow or carping spirit should we approach the work of one who has done so much for the cause of evolution. We may set our queries against this or that matter of detail, or may enter a protest against the acceptance, without more conclusive proof, of certain broader principles; but we should not dwell on minor defects nor allow more grave differences of opinion to blind us to the gift which Mr Herbert Spencer placed in our hands thirty-four years ago, and has borrowed for awhile that he may return it to us with added weight.

The phrase 'added weight' is perhaps ambiguous. The volume of the work is materially increased, and new sections of much interest have been added. But though the intellectual weight has also been augmented, it is an open question whether it would not have been wiser to leave intact a treatise of such unique historical importance and value, relegating corrections and additions to notes and appendices. With all the labour and care Mr Spencer has expended on it during the last two years, it cannot be said that, having due reference to the contemporary state of knowledge in each case, the revised volume of 1898 holds the same position as the original work of 1864.

The scheme and method of the Principles of Biology and the manner in which Mr Spencer develops his subject are presumably so familiar as to render anything more than the briefest summary unnecessary. Starting in Part I. with the Data of Biology, the composition and properties of organic matter, the actions of external forces on it, its reactions to these forces, and its characteristic modes of metabolism, are severally discussed, and the conception of Life,

[1] "The Principles of Biology," by Herbert Spencer, vol. i. Revised and Enlarged Edition. 8vo, xii+706 pp. London: Williams & Norgate. 1898. Price 18s.

as involving the continuous adjustment of internal relations to external relations, is developed. To this part a new chapter on the Dynamic Element in Life is added.

After an indication of the scope of biology, the inductions of the science are considered in Part II. Generalizations as to growth, development, adaptation, genesis, heredity, and variation, are formulated and illustrated; the classification and distribution of organisms are considered, and the foundations are thus laid for the erection of an aetiological superstructure. The special-creation hypothesis is contrasted with that of evolution; the arguments for the latter are marshalled; and the causes of evolution discussed. Internal and external factors are distinguished; and the phenomena are explained as due to the joint action of (1) direct equilibration through the inheritance of acquired modifications, and (2) indirect equilibration through the survival of the fittest in the process termed by Darwin 'natural selection.' A concluding chapter on Recent Criticisms and Hypotheses, in which any inherent tendency to evolution along predetermined lines is rejected, brings the volume to a conclusion, save for Appendices, amongst which are the *Contemporary Review* articles on the Inadequacy of Natural Selection.

In the additional matter of the present edition Mr Herbert Spencer would probably lay most stress on the chapter which deals with the Dynamic Element in Life, and on the arguments in favour of direct equilibration through the inheritance of acquired modifications. While other additions, such as those on protoplasm, on metabolism, on nuclear changes, on embryological development, and on classification, serve mainly (for there are original suggestions) to bring the work into line with modern biological conclusions, the supplementary discussion of the Dynamic Element in Life is in touch with the author's distinctive philosophical tenets, and the arguments for direct equilibration are adduced in support of biological conclusions which Mr Spencer regards as of extreme importance. It seems desirable therefore to direct attention specially to these points.

After leading up to a conception of Life as the definite combination of heterogeneous changes, both simultaneous and successive, in correspondence with external co-existences and sequences, and after urging that the degree of life varies as the degree of correspondence, Mr Spencer briefly indicates certain vital processes which remain outside the conception as thus formulated, and contends that all cases "exhibit that principle of activity which constitutes the essential element in the conception of life." This he terms the dynamic element in life; and he asks whether it is inherent in organic matter or is something superadded. The notion of a super-

added vital principle is rejected. To the questions: Is there one kind of vital principle for all kinds of organisms, or is there a separate form for each? How are we to conceive the genesis of a superadded vital principle? Under what form does it exist in the dessicated rotifer?—to these questions the answers show that the alleged existence neither has been nor can be conceived. In attempting, on the other hand, to realize the dynamic element as "inherent in the substances of the organisms displaying it, we meet with difficulties different in kind but scarcely less in degree. The processes which go on in living things are incomprehensible as results of any physical actions known to us." "What then," he asks, "are we to say—what are we to think? Simply that in this direction, as in all other directions, our explanations finally bring us face to face with the inexplicable. The ultimate reality behind this manifestation, as behind all other manifestations, transcends conception."

One must not forget, in reading the chapter on the Dynamic Element in Life, that it forms part of a work which is itself only a part of a System of Philosophy. In Biology as a science it is questionable whether reference to the Ultimate Reality, and to noumenal as contrasted with phenomenal causation, is advisable. Phenomenal causation, as an explanation of natural occurrences, involves the reference of an event to a group of antecedent conditions of which it is the outcome. Noumenal causation, as an explanation of the totality of natural phenomena, involves their reference to an underlying *raison d'être*. The one deals with a chain of antecedents and sequents the ends of which, and its manner of support, are beyond the range of our mental vision as men of science. Of the other we can at best know or assume that it is. The phenomenal universe presents us with, or rather is, a series of data. Science explains their connections, and leaves to philosophy a discussion of their noumenal origin. But since Mr Spencer here treats Biology as part of a System of Philosophy the ascription of phenomena to their noumenal origin is not out of place, though one would have thought that a reference to "First Principles" should have sufficed.

And one cannot but think that Mr Spencer's treatment of the subject may render him liable to some misconception. On the first page of the "Principles of Biology" there stands now, as there stood in 1863, the assertion that "the properties of substances though destroyed to sense by combination, are not destroyed in reality. It follows from the persistence of force, that the properties of a compound are resultants of the properties of its components—resultants in which the properties of the components are severally in full action though mutually obscured." Further on we are told (in sections added to the present edition) that living matter "originated, as we must assume, during a long stage of progressive cooling," in which

occurred "the formation of molecules more and more heterogeneous." The inference which may fairly be drawn is, that the properties of living substance are the resultants of the properties of its components. But at first sight this does not seem to square well with the assertion that "the processes which go on in living things are incomprehensible as the results of any physical actions known to us." One can picture how certain folk will gloat and "chortle in their joy" over this confession, for such it will almost inevitably be regarded. But it is not likely that Mr Spencer is here, in so vital a matter, false to the evolution he has done so much to elucidate. The two seemingly contradictory statements are not really contradictory; they are made in different connections; the one in reference to phenomenal causation, the other to noumenal causation—to an underlying "principle of activity."

The simple statement of fact is that the phenomena of life are data *sui generis*, and must as such be accepted by science. Just as when oxygen and hydrogen combine to form water, new data for science emerge, so, when protoplasm was evolved, new data emerged which it is the business of science to study. In both cases we believe that the results are due to the operation of natural laws, that is to say, can, with adequate knowledge, be described in terms of antecedence and sequence. But in both cases the results, which we endeavour thus to formulate, are the outcome of principles of activity, the mode of operation of which is inexplicable. We formulate the laws of evolution in terms of antecedence and sequence; we also refer these laws to an underlying cause the noumenal mode of action of which is inexplicable. This, if I interpret him rightly, is Mr Spencer's meaning.

The use of the phrase 'dynamic element in life' is also open to misconstruction. A dynamic element, as understood by science, is a link in the phenomenal chain, and is expressed in terms of the inter-relation of the parts of a material system, and of actually observed attractions and repulsions. But it is customary for physicists to introduce at the outset of their discourse definitions of force as the cause or *raison d'être* of motion. These are now commonly regarded as a pious tribute to the noumenal, like grace before meat, and independent of physics in its strictly scientific aspect. But since such a definition of force not infrequently stands in the fore front of a treatise on dynamics, it may be regarded, noumenal though it be, as a dynamical postulate. It is in this noumenal sense that the term 'dynamic' is used in Mr Spencer's phrase. It is, we are told, a "principle of activity." And unless the reader is careful to note the distinction, Mr Spencer's position will be open to misconstruction. Vitalism is a philosophical conception; and the controversy which is suggested by the term is largely due to the

failure of the controversialists to distinguish carefully between noumenal and phenomenal causation.

The question may now be asked whether Mr Spencer is well-advised in attempting to comprise under one definition of life (1) certain fundamental attributes of living matter and (2) sundry complex results of evolution. Is not the correspondence between life and its circumstances rather the result of the interaction of vital factors, under the influence of the environment, than a primary characteristic of the vital factor itself? And if it be true that the degree of life varies as the degree of correspondence (which is open to question, since it is rather struggle than attainment which calls forth most strenuous vital energy) it is surely aggregate results, rather than fundamental properties, that we have in mind. If a unit must be selected, the cell, not the metazoon or analogous plant-complex, seems to be the more appropriate. But it is perhaps more profitable to fix attention, not on the variable unit, but on the common substance—protoplasm; to say that protoplasm has certain specified metabolic or other properties, that the processes carried on in virtue of these properties are what we comprise under the term life, and that evolution is the outcome under certain given conditions.

If Mr Spencer reply that the outcome in evolution is itself a fundamental attribute of life, it is difficult to see, in view of his position with regard to the dynamic element, on what *a priori* ground he criticises the hypothesis of determinate evolution—the orthogenesis of Eimer. To argue that the evidence of such determinate evolution is insufficient is a perfectly legitimate scientific position. But Mr Spencer goes further. He says: "The assertion that evolution takes definitely-directed lines is accompanied by no indication of the reasons why particular lines are followed rather than others. In short, we are simply taken a step back, and for further interpretation referred to a cause said to be adequate but the operations of which we are to imagine as best we may." If, however, we are to believe in a dynamic element in life, itself inexplicable, and if the outcome in evolution is in itself a fundamental attribute of life, it is difficult to see why the inexplicable determinate tendency should not be part and parcel of the inexplicable operation of the dynamic element. But if we regard evolution as the result of the complex interaction of vital factors, then we may fairly demand an explanation of the manner in which such interaction can give rise to orthogenesis.

It would be unprofitable (even if space permitted) to discuss at any length the pros and cons of what Mr Spencer terms direct equilibration. He is deeply committed to the inheritance of acquired modifications, and on this hinges much of his interpretation,

not only of biological, but also of psychological and sociological phenomena. But though this must to some degree influence his judgment, he is too honest and independent an enquirer into truth to champion a cause on these grounds alone. The pendulum of biological opinion tends to swing towards a negative position in this matter; and it is a distinct gain that the arguments in favour of the doctrine in question should be presented with all the logical force and power of exposition which Mr Spencer has at his command. And if there are many who still remain unconvinced, this is not for want of fresh arguments but because they feel the necessity for more facts.

Of the three lines of evidence on which Mr Spencer in large degree relies, the first is co-adaptation of co-operative parts. But until we know more accurately than we do at present what amount of co-adjustment is effected in each case by individually-acquired modifications, we lack important data for discussing the problem. Probably its range is very considerable. The horse runs and leaps with the added weight of his rider; and the work of all domestic animals of draught and labour shows that their organization will stand a strain far in excess of the normal. Granting that individual coadjustment will in each generation do much (just how much remains to be proved) it would seem that in the case of evolving antlers the added weight will for long be within the limits of such individual coadjustment. But it has been urged that the modification of a structure may foster, though it may not cause, congenital variations of like kind. For whereas other variations, since they are out of harmony with the circumstances of life, will constantly be eliminated in the struggle for existence, these are allowed free play. In other words, congenital variations coincident in direction with acquired modifications will be favoured. And there is no necessity for them to be accurately simultaneous; individual co-adjustment will make good the deficiencies of congenital co-adaptation. In view of such considerations the argument from coadaptation has little weight till we have fuller knowledge of the range of coadjustment.

The second line of argument adduced by Mr Spencer is the possession of unlike powers of discrimination by different parts of the human skin. But here again we need more facts. It is assumed that this discrimination is largely congenital. But we do not know what proportion of it is individually acquired. We do know that a comparatively short period of education in little-used areas largely increases, is said to double, the power of discrimination. And some psychologists contend that—as the term discrimination implies—what we are really dealing with is the special application of the power of central perception, not an increased delicacy of

peripheral nerve-endings. There is no spot on the skin that is not sensitive to the touch of a pencil-point. And we do not yet know the limits within which education and practice may refine the application of central powers of discrimination within little-used areas. The facts which Mr Spencer adduces may be in large degree due to individual experience; discrimination being continually exercised in the tongue and finger-tips, but seldom on the back or breast. We need a broader basis of assured fact.

It may here be parenthetically noted that Mr Spencer's contention that the nervous system is the result of direct equilibration is difficult to square with the embryological discovery that the axis-cylinders of the afferent spinal nerves take their origin in the nerve-crest (which differentiates into the ganglia on the dorsal roots) and grow outwards to their distribution in the skin or elsewhere.

A third line of evidence on which Mr Spencer relies is that supplied by vestigial organs, which, he contends, must be due to dwindling through disuse. But the explanation is beset with difficulties. Is there any evidence that a structure really dwindles through disuse in the course of individual life? Let us be sure of this before we accept the argument that vestigial organs afford evidence that this supposed dwindling is inherited. The assertion may be hazarded that, in the individual life, what the evidence shows is that, without due use, an organ does not reach its full functional or structural development. If this be so, the question follows: How is the mere absence of full development in the individual converted through heredity into a positive dwindling of the organ in question? In our present state of ignorance we can only adopt the form used by Mr Spencer and say: No reply.

It will be understood that the foregoing considerations are urged, not in support of one hypothesis or in opposition to another, but in advocacy of suspended judgment, of calm and impartial weighing of evidence, and, above all, of further observation and experiment. While the forces of battle are arrayed under the banners All-Sufficiency of Natural Selection and Inheritance of Acquired Modifications, there are non-militant biological agriculturalists who till the fields of observation and assert that they in truth provide the sinews of war. It is to them that we must look for conclusive evidence one way or the other.

We cannot part with Mr Spencer (only for a time it is hoped) without again expressing sincere admiration for his genius and gratitude for his self-sacrificing labours.

C. Lloyd Morgan.

University College, Bristol.

II

Artificial Formation of a Rudimentary Nervous System [1]

(*d*) **Inhibition.**—Cl. Bernard's theory of nervous interference, can be corroborated by producing two excitations of an almost equal intensity at the ends of a big thread of mercury and by putting two halves of a tube of caoutchouc in the midst of the thread's surface. This tube performs the part of a heart, because its halves alternately approach and part at the passage of the waves. (Fig. 8.) Rest is doubtless the issue of the wave interference. (Action of the internal branch of the spinal.)

(*e*) **Formation of the dilated parts of the nervous system by means of wave interference.** — The continual excitations applied to both ends of the thread of mercury, originate the formation of a central dilatation. (Fig. 8′.) It, then, seems probable that the ganglions, plexus and dilated parts of the embryonic system, whose consistence appears to be even softer than that of adults, are owing to the continuous shocks of the vibrating waves that cause the unequal distribution of nutritive materials or the movement and concentration of the already constituted parts. Now, if the consistence of the neuroplasma increases, one may be sure that the construction of the definitive dilated parts has been finally attained.

This is, in fact, an extremely important cause of differentiation and consequently of progress. In higher animals the most continuous and intense sensations terminate in an excessive division of the nervous elements confined to the neuroglia. I have discovered that the consistence of the latter presents an exceptional importance. For instance, if you wish to obtain a great number of multipolar cells anastomosed almost in the same manner as those of the gray substance, you have but to place on some lard any viscous liquid (coloured albumen, saliva).[2] Even the action of a terminal resistance on a big thread of mercury (augmentation of consistence, inclination, etc.) suffices to obtain the claviform or cerebriform shape.

(*f*) **On the action of moderating nerves.** — These are

[1] Continued from p. 339. The figures referred to are on p. 334.
[2] Models of multipolar cells. *Natural Science*, August 1898.

probably traversed by some wave-currents endowed with a celerity that gives them, in some measure, the means to oppose themselves to the currents coming from the ganglia of the sympathetic system as well as to those issuing from the viscera. Some disordered movements of the heart, intestines, etc., are naturally the result of the suppression of the action of the brain and medulla. This theory ought to be applied in cases of vascular inhibitions.

(*g*) **On the part performed by the medulla.**—" Les résultats contradictoires de plusieurs physiologistes au sujet de la fonction des cordons antérieurs et latéraux de la moelle, tenaient aux modes divers d'excitation mis en usage. Vulpian a constaté qu'il faut une excitation très énergique pour déterminer les contractions dans les muscles recevant leur innervation des parties situées au dessous du faisceau excité : que les attouchements, les piqûres, les grattages superficiels ne produisent aucun résultat, mais qu'on met en jeu l'excitabilité de ces faisceaux en les pressant entre les mors d'une pince." [1] That is, until a powerful wave is produced. It is just the same with large masses of mercury, the vibrations of which cannot be brought about by rubbing them with a soft feather, and much less when the latter is protected by a cover.

" La substance grise de la moelle ne conduit point les impressions sensitives par des voies anatomiquement préétablies, mais pour ainsi dire d'une manière indifférente. Les sections transversales peuvent diviser la moelle epinière dans une grande partie de leur épaisseur, et dans un sens quelconque, sans interrompre la transmission des impressions sensitives, à la condition qu'une petite partie de la substance grise (une sorte de pont) ait été respecteé par l'incision. L'animal conserve la possibilité de reconnaître le point du corps irrité. Vulpian parlait d'une sorte d'empreinte originelle des sensations. . . ." The question is quite a simple one. The differences are only in the intensity of the vibration occasioned by the variability of distance, degree of excitation, point on which it worked, protecting envelopes, etc., and as a matter of course the excitations are conveyed almost in the same manner and each of them reaches certain points of the sensorium, however small the bridge of the gray substance may be.

Make a sort of a medulla with mercury and insert some conductors on its surface. (Fig. 10.) Some multipolar cells and conductory threads are then to be disposed in its upper part, as in Luy's methods. A stronger excitation will then rise higher than a weaker one and the elements placed at different heights will be diversely effected. Each of these reacts on the motor threads, but only in case it be sufficiently excited. (Fig. 10.) In this manner the perception of simultaneous sensations and multiple reactions

[1] Küss and Duval, l.c., p. 67.

are elucidated. For example: if you have a burning on your hand and another on your foot, they will be directly followed by two corresponding movements of retraction.

(*h*) **Reflexes or reflected circulation** (see fig. 11).—My theory alone can explain the law about the symmetry of reflexes (fig. 10), as well as those concerning intensity, irradiation and generalisation: this means that as the mechanical excitation grows stronger the vibration propagates itself more or less and produces a more and more general reaction. The same explanation can probably be applied to the phenomena of eccentricity. Any associated sensations, for instance the tickling, cough and nausea provoked in the pharynx by the pressure of a strange body in the ear, are to be explained by an excitation so strong as to radiate to the immediate centres of reflection.

Association of ideas consists perhaps but in the fact of a successive vibration; whenever some elements vibrate, such as stand near are set in motion by them.

"All our voluntary motions are in general associated, since we cannot move a single muscle separately but must needs move a group of them." And why so? Because the vibrations of the anatomic elements tend inevitably to radiate, notwithstanding inertia. This is observed in capillaries of mercury when examined under the microscope.

(*i*) **Influence exercised by the mass.**—Milne-Edwards says that the stronger an animal is the more mass its nervous system holds. This is made manifest in fig. 12.

(*j*) **Persistence of impressions.**—It is but natural that an excitation endowed with some intensity should produce a vibration that only dies away gradually. (Fig. 15.) Several optical delusions are probably owing to this cause.

(*k*) **Theory of sleep.**—The multipolar cells of the centres are known to have amoeboid motions and to articulate and disarticulate themselves (fig. 19) by the discharge of CO^2(?). This is exhaled in less proportion during sleep.

(*l*) **Model applicable to a general conception of a nervous system.**—I constructed a model in mercury, of the circulation of sensations and concomitant phenomena, modifying that (p. 48) presented by Luys in his great work on the brain. The most curious part of it all is that the vibrations provoked in the conductors are only reflected with sufficient intensity on the cells standing nearer the sensorium, and that they pass from thence to the big interior cells. There seems therefore to be no complicated mechanism whatever, everything appearing to result from a mere question of strength and distance. (Fig. 20.)

(*m*) **Evolution of the nervous system.**—The following experi-

ments need to be performed in a dark room, or at night, by illuminating the liquid with the somewhat oblique rays of a candle so as to obtain a marked shadow. Fill a floating dish with water and pour on its surface a small quantity of petroleum. (Fig. 21.) This forms large drops, on the edges of which appear certain pseudopodia with terminal spheres: these advance inwards originating therein several curious phenomena which are of no consequence at present.

The drops of petroleum playing the part of the neuroplasma or that of the nervous elements in evolution, project a distinct shadow on the white ground of the dish. Water is the actor intrusted with the all-important part of the neuroglia.

Some other liquids may likewise serve the purpose, since there is nothing indispensable, except the question of their densities and the indissolubility of the one in the other.

In fig. 21 there are five large masses of neuroplasm scarcely differing from each other (young foetus). But when any waves are produced in a single direction (acoustic impressions for instance) *A* and *B* are divided (fig. 22) and six different elements result from this mechanical division. Then, whenever the vibration continues with increasing intensity, one of the drops lengthens, adhering by one of its sides to the neuroglia or water, and finally constituting a kind of myelocyte, undergoes tension in several directions and sends the element *c* forth. We now have eight drops instead of the five primordial ones and a very small one, a little like the nervous embryonic cells (fig. 23). Still the experimenter goes on his task of differentiation and provokes many waves in two directions, obtaining thirty-seven more or less deformed drops (fig. 24). To close the experiment a new undulatory motion is provoked with a thick pin in a single direction and some currents of bipolar, multipolar, apolar, and articulated cells are formed, some resembling neuromata, while others are elliptical and have nuclei in them. (Fig. 24.) Though these figures are transitory I believe that permanent ones could be obtained by employing a melted grease which would preserve the shapes acquired by vibration as it cooled. The vibration can be provoked either in the water or in the floating drops of petroleum themselves.

Conclusions.—The origin of individuals and the construction of the organism by internal conditions is an exceedingly probable principle; even the origin and functions of the nervous system may generally be explained by vibrations, by waves running through certain conductors of neuroplasma and modifying it mechanically with regard to its shape, division, and connections. Every cause influencing general nutrition will modify the physical and chemical properties of the neuroplasma and of the neuroglia in whose bosom it slowly performs its boundless evolution.

Haemorrhages, commotions and circulatory disturbances; changes in the density and chemical composition of the blood, in the fecundating principle of the nerve which excites and awakes it just as the nitric or chromic acid set the mercury trembling; bile, that is a great excito-motor ; inanition, inertia and fatigue ; all that alters the compass of the vaso-motory systems or the nutrition of the neuroglia or of the neuroplasma may conduct to idiotism or frenzy.

There is a rise and fall also, an immense waste of nervous complications resulting from a small, simple, mechanical event. Now, when any shapeless, primordial masses of neuroplasma are for ever vibrating and dividing themselves, be it by work, by exalted excitations of the sensorial impressions, by innumerable congestions, or by hunger, love, strife, meditation, millions of small, light, plastic, movable cells issue, that are incessantly articulating and dislocating themselves. Intellectual perfection could otherwise not be conceived. Broca observed that when the faculty of speech was lost by alterations of the frontal centre, it could be gradually recovered by a development of the opposite hemisphere. In short, the evolution of which the encephalon of man and animals is susceptible by means of education, demonstrates that the systems and divisions of nervous elements are not invariable, but perfectible and variable, and that the neuroplasm keeps for a longer or lesser time the shapes acquired, according to the degree of its density and vigour, several circumstances, such as age, vivacity of first impressions or vibrations, its repetition, and so forth, being at all times prevailing.

Physiologists evince an inveterate electro-mania in their exertions to explain everything by the action of electricity, taking the negative variation together with the fact of the existence of electric fishes as a principle. Well, notwithstanding the minute investigations of Du Bois Reymond and others, they have never been able to explain anything by such means, not even the influence of compression upon the nerve, or the muscle's vibration. Besides, Marey and Moreau have demonstrated that the electrical apparatus of the *Torpedo* works as a muscle and has almost muscular jerks ; second, that the nerves with which it communicates do not carry the electricity to this queer machine, having only the faculty to set it to work. Moreau proves that when the prisms are treated with various reagents the discharges are not modified, but that they come to a stop when the former coagulate the albuminoid bodies, that is, soon after the physical conditions of the phenomena are modified. I suspect that nervous vibration has a mechanical action there, and that electricity unfolds itself on account of some rubbing or vibrations in the separating partitions of the 100,000 or 200,000 close-packed prisms. Becquerel says it is enough to press a disc of cork on

an orange and to separate it rudely afterwards to load it with a considerable quantity of positive electricity.

. . . . . .

I conceive the human organism, therefore, as a machine containing some five or six litres of blood employed in appropriating to itself the nutritious principles of food, absorbing oxygen, and carrying it to the nerve to make it vibrate by discharges of carbon dioxide.

Yet, this machine being magnificent, it were suitable to imitate it by means of some very sensitive springs (muscles) moved by the vibrations of a great number of semi-liquid conductors (nerves) in communication with proportionated deposits or centres.

The inheritance of nervous affections or that of the attributes of genius may, by consequence, be easily explained. Whenever the progenitors have a strong, healthy, and active protoplasma, the nutrition of the embryonic neuroplasma of the descendants is perfect. All will, in fact, be correlatively nourished, and the germs united by molecular attraction will form a strong gastrula, a superb neurochord, and later still the three fundamental embryonic brains.

Féré's observation regarding the transmission of such nervous affections, epilepsy and the like, as manifest themselves so soon as the general conditions of nutrition improve, can be elucidated thus, and likewise the singular immunities of wise men as indicated by foreign physiologists, and Chalumeau's famous law.

A. L. Herrera.

Mexico, *May* 1, 1898.

[It should be explained that Professor Herrera wrote this article in a language with which he is unfamiliar, and that he has had no opportunity of correcting the proofs.—Ed. *Nat. Sci.*]

III

The Neuration of Rhopalocera

GIVEN the theory of evolution, structural characters of living insects, whether embryological or imaginal, afford means of classification according to the persistence among them of various degrees of specialization from primaeval forms of structure, *i.e.* the links of continuity of specialization in any direction. The neuration of the wings is a structural character common to all Lepidoptera —a few apterous females excepted. Hepialides and Micropterygides, perhaps the most ancient groups of Lepidoptera now existing, have more wing nervures than any other group and associate the Lepidoptera with Trichoptera; series of gradations in modification of neuration in different directions more or less connected (less connected, perhaps, among ancient than recent groups), may be observed everywhere among existing Lepidoptera; thus reduction in the number of nervures, or alteration in the position of certain nervures, connect generalized (ancient) and specialized (recent) forms of neuration; as a matter of observation, I believe that, nervures once lost, or the position altered, neither has ever been regained—hence the different series of gradations.

Fore and hind wings corresponding, the neuration may be referred to as follows:—Costa = upper margin. (1) Subcostal nervure. (2) Radial system = nervure with branches. (3) Median system = nervure with branches (nervules). (4) Cubital system = nervure with branches (nervules). (5) Anal nervures = several simple (unbranched) separate nervures.

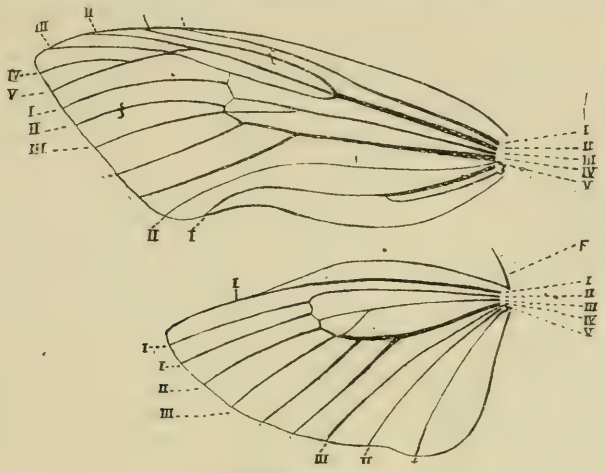
In this paper I propose mainly to refer to the neuration of the forewings, and possibly shall not enumerate all the nervures of the wings to save unnecessary details.

Cossids, so far as I know, cannot be considered in any way related to the Rhopalocera, except by connections too remote to trace; they retain a neuration relatively more ancient than that of any group of Rhopalocera.

It may be instructive to compare the Cossid form of neuration with the several more or less definite forms representing several groups of genera in the Rhopalocera, and I may here say that by the term group I mean an assemblage of species which may be associated upon a given pattern of wing neuration, or a distinctly connected modification of the same. In a recent instructive paper upon the "Classification of the Day Butterflies," published January and February 1898 in *Natural Science*, Dr Radcliffe Grote has given details of

the modification of neuration to be observed in the different groups, and I will briefly refer to some lines of specialization observed.

Group 1.—Generalized Hesperids (*Hesperia*) retain five-branched radius in forewings, and three median nervules in very nearly the ancestral position; but a modification of the transverse 'cell' nervure in the direction of the cubital system forms a three-branched cubitus in forewings (ancestral form is two-branched as in Cossids). Among



Australian Cossid.

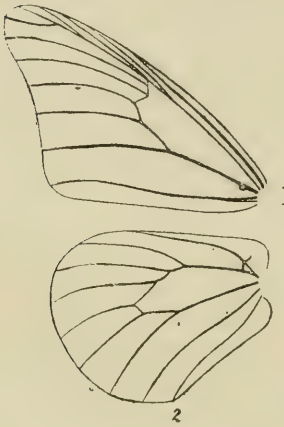
specialized Hesperids (*Pathesperia*) one median nervule by incorporation with the radial system forms a six-branched radius in forewings, the middle median nervule and the cell nervule in hindwings disappear, and there is a tendency in the middle median nervule in forewings to do likewise. (See Dr Grote's figures; *Nat. Sci.*, vol. xii., pls. i. and ii.).

The neuration of Lycaenids is similar to that of Pierids, but not identical; in each group specialization by reduction in the number of radial nervules in forewings is observed, but there is this difference, viz., whereas among Lycaenids reduction occurs while the median nervules retain their position and identity as such, the same specialization is observed among Pierids after the incorporation of one of the median nervules with the radial system. Whether it is possible to take into consideration the relative position of the median nervules in the forewings of Lycaenids and associate them with Hesperids therefor, my personal observations are not sufficiently extensive to justify an opinion. Certainly the position of the median nervules is extremely alike in each group, and Dr Grote associates them, having observed also the tendency among Lycaenids to lose the middle median nervules in the same manner as do Hesperids. This, so far as

Chrysophanus phlaeas (Lycaenid).

I know, has not been observed in any other group. The neuration of Lycaenids is specialized compared with that of the generalized Hesperids, and sufficiently distinct from that of Pierids and allied groups to warrant the conclusion that the latter are as far removed from Lycaenids as they (Pierids, &c.) are from Hesperids. Lycaenids and Pierids have attained a similar specialized form of neuration independently and by different routes. Grote writes, "Lycaeni-Hesperidae meet upon a distinctive wing pattern, the Lycaenidae differ in the main by the reduction of the radial branches."

Group 2 (A).—Generalized Nymphalids, (Danainae, &c.) retain five-branched radius, have distinct three-branched cubitus in forewings, and retain the 'discoidal cell' transverse vein of each wing. *Anosia* has a structural 'blotch' (A) towards the base of the cubital system, and a rudimentary nervure (B) at the base of the anal nervure in forewings. The latter feature is observed in other groups, and occurs frequently among the Heterocera. *Anosia* has also a subcostal-radius connection in hindwings (C). Specialized Nymphalids show a gradual loss of the transverse 'cell' nervure of each wing, complete loss being attained (*Apatura*, *Junonia*, &c.) without incorporation of the median nervules with the radius system of the forewings—the two median nervules remain as such, attached to the radius by a modified remnant of the 'cell' nervure.



Anosia archippus
(Nymphalid).

Enodia hyperanthus
(Satyrid).

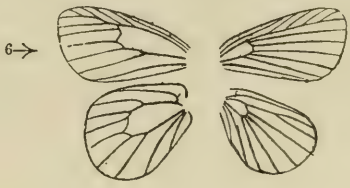
Satyrids may be associated by descent with generalized Nymphalids; the radius system is five-branched, and the 'cell' nervures are retained throughout the group, the wing pattern is distinctly nymphalid. The features A, B, C of *Anosia* are not observed among Satyrids.

Junonia orithya
(Nymphalid).

Group 2 (B).—Generalized Pierids. *Leucophasia* is isolated among the Rhopalocera in respect of the wing pattern, and the neuration must be regarded as specialized, especially in regard to the arrangement of the radial branches (five) of the forewings. As a Pierid, however, having five-branched radial system, two median nervules, and three-branched cubitus in forewings, *Leucophasia* affords a connection between the generalized Nymphalid form of neuration, and

the next in sequence among the Pierids, viz. *Euchloe*, in which incorporation of one of the median nervules with the radial system of the forewings forms (as in Hesperids) a six-branched radius.

Pieris daplidice.

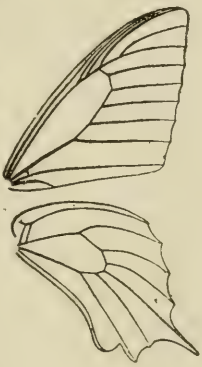


Euchloe cardamines (Pierids.)

Leucophasia sinapis.

Further specialization among Pierids is by reduction in the number of radial nervules in the forewings, and the movement of the remaining median nervule towards the radial system. The transverse 'cell' nervure is a permanent feature throughout the Pierid group.

Group 3.—Papilionids. The forewings have five-branched radial system, four-branched cubitus, only one median nervule remaining as such, a transverse cubitus and connection near the base of the wings, and two anal nervures. The hindwings have a subcostal-radius connection, a very persistent feature, which varies only so far as to form larger or smaller 'cells' in different genera. One anal nervure only is typical of Papilionid neuration. All other groups (*i.e.* Groups 1 and 2) have two anal nervures in the hindwings.



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Papilio machaon (Papilionid).

From these notes it will be seen that however widely the specialized Rhopalocera may differ, the generalized forms of neuration have features in common—five-branched radius, two median nervules as such, three-branched cubitus in forewings (Groups 1 and 2). Dr Grote believes it is "uncertain" that the short anal nervure of Papilionid forewings, is homologous with the rudimentary anal nervure in the forewings of other groups, and conceives a separate origin for Papilionids. In such groups of Heterocera as have two anal nervures in the forewings, a rudimentary nervure is often present (see Cossids), always at the base of that anal nervure farthest from the cubital system. Comstock figures *Megalopyge crispata*, a 'low' moth which has an extension of the rudimentary nervure, a remnant of a third anal nervure in the forewings. Most Rhopalocera have lost one anal nervure (nearest the cubital system), a loss observed in many Heterocera; Papilionids retain two, one of which has become rudimentary in other groups of Rhopalocera (also in Cossids and many

Heterocera). Here also we may refer to the features A, B, C of *Anosia* (evidently overlooked by Dr Grote); as already stated, these are not to be observed among specialized Nymphalids. Now, the cubitus-anal connection, short anal nervure of the forewings, and subcostal-radius connection in the hindwings of Papilionids are generalized features, homologous with the features A, B, C of *Anosia*, modified and lost in other groups of Rhopalocera; it is impossible to regard them in any other light. In this respect then Papilionids approach the generalized Nymphalids.

Reaction of modification appears to me to have caused different lines of specialization of neuration in the Lepidoptera, consistent with adequate support of the wing membrane, and the development of wing area and shape; for example, it seems that modification of neuration has been rapid in the case of *Opostega crepusculella*, which is not highly specialized in wing area and shape as compared with primeval Trichoptera-Lepidoptera, but the neuration has been reduced to several simple (unbranched) nervures. The area and shape of Rhopaloceran wings are, however, more highly specialized, and the modification of neuration—particularly of the forewings—has been less rapid. If we draw the line separating primitive Rhopalocera from the mass of primaeval Lepidoptera at Hesperids, then we have the fact that no Rhopalocera possess the basal portion of the median nervures, and this loss we may assume to be the point of separation (granting, also, modification of hindwing neuration, which everywhere in the Lepidoptera has preceded that of the forewings). Next we have noted the formation of three-branched cubitus in the forewings, by the incorporation of one of the median nervules with the cubital system in Hesperids and subsequent specialization in the direction of Lycaenids. In another direction, apparently at some distance from Hesperids, we note that a three-branched cubitus is a fixed feature in the neuration of Nymphalids' forewings, that no movement has taken place in the position of the other median nervules, and very little subsequently among Nymphalids and Satyrids. But as the Pierids separated from the Nymphalid-Pierid stem, the median nervules moved towards, and one became incorporated with, the radial system. The cubitus 'blotch' and rudimentary anal nervure of the forewings and the subcostal-radius connection in the hindwings of *Anosia* point to the possession of the Papilionid cubitus-anal connection, two anal nervures in the forewings, and a subcostal-radius connection in the hindwings by those primaeval Rhopalocera from which the Nymphalid-Pierid groups originated; and another important fact is that at this point two anal nervures in the hindwings were retained (as in Hesperids). Next in sequence come the Papilionids, in which movement of one of the (Nymphalid) median nervules forms

a four-branched cubitus in the forewings, the generalized cubitus-anal connection, and the two anal nervures of the forewings being retained, likewise the subcostal-radius connection of the hindwings. One anal nervure only, however, is retained in the hindwings; that is to say, one of those present in Nymphalid-Pierids has been lost.

My conclusion from a study of the neuration is that the distinct separation of Papilionids in phylogeny from the other Rhopalocera cannot hold good, and in this I join issue with Dr Grote. Whether I have clearly demonstrated this in the above, I cannot say; but the facts are distinctly in evidence that, however widely the specialized forms differ, the generalized forms of neuration indicate a natural sequence in modification—the evolution of the Papilionid form from a primaeval form such as is illustrated by Cossids; and of this evolution the generalized Hesperids (1), Nymphalid-Pierid (2), and finally generalized Papilionids represent the surviving links in the continuity of specialization, a primary modification of neuration antecedent to and quite apart from the special modifications peculiar to the several groups.

I need only refer to Dr Chapman's paper, "Butterfly Pupae" (*Entom. Record and Journal of Variation*, vol. vi. pp. 106 and 125), to support my conclusions: "The lowest (*i.e.* most ancient) forms in all the families are really very close together."—"Pierid and Nymphalid started together, shortly afterwards separating." Nymphalid pupae are capable of lateral movement only, as are Pierids, but have lost the 'girth' which is characteristic of the Pierid and Papilionid method of pupation; Nymphalids, however, retain the 'double nosehorn' pupal structure of Papilionids (*i.e.* a generalized pupal character), Pierids having lost this particular pupal character. The 'double nosehorn' is surely corroborative of my conclusion that the features A, B, C of *Anosia* are homologous with the Papilionid cubitus-anal connection, short anal nervure in forewings, and subcostal-radius connection in hindwings, which establish affinity between the Nymphalid-Pierid and Papilionid groups.

If Papilionids be separated from Nymphalid-Pierids in the manner proposed by Grote, then the neuration of Papilionids must be regarded as isolated, highly specialized, and without surviving forms connecting it with primaeval neuration from which it has been derived. On the other hand, the "primary modification of neuration" among primitive Rhopalocera shows the formation of the three-branched cubitus in forewings, with two anal nervures in hindwings; then follows the formation of the four-branched cubitus, with loss of one anal nervure in hindwings. This is more in accord, I believe, with the evidence of development, and supplies the connecting links between the primaeval neuration and that of the highly specialized Papilionid.

A. Quail.

Palmerston North, New Zealand.

IV

A Theory of Retrogression

IT is widely believed that the development of the individual is a recapitulation of the life-history of the race. In other words, it is believed that every individual begins life as a unicellular animal, the germ, and then, in a very rapid indistinct fashion, represents, in orderly succession, all its long line of ancestors, till in the end it represents its parent. This recapitulation is not more wonderful and mysterious than any other fact of biology. Imagine the primitive world, in which only unicellular organisms were present. Suppose that variations occurred amongst these, just as we know they occur higher in the scale. Then we may well believe that such variations as the following occurred—that, when one cell divided into two, the resulting cells did not separate, as normally happened, but remained adherent. This variation, which, like other variations, would tend to be transmitted, and which, if fortunate, would tend to cause the ultimate survival of the organisms which possessed it, would be the first step in the evolution of the multicellular from the unicellular organism. The dual animal which resulted would reproduce by each of its cells dividing into two, so that there would be four single cells which would separate, so as again to form unicellular organisms. But each unicellular organism would, in general, inherit the peculiarities and repeat the life-histories of its grandparent cells by dividing into two adherent cells. A race of two-celled organisms would thus be established. We may fairly believe that in time a second variation, which also proved fortunate, occurred, whereby the four grand-daughter cells also remained adherent until reproduction; and afterwards other variations of the like nature, till an organism was at length evolved which consisted of a multitude of cells adherent for the common benefit. When this organism reproduced it would be by one or more of its cells separating and dividing into two adherent cells, these into four, and so on, till the parent organism was represented. Ontogeny would thus necessarily recapitulate phylogeny. This rule would still obtain when evolution proceeded farther, and cells had become differentiated and specialised for the performance of different functions. Every individual would still begin as a single cell, the germ, and then, step by step, would represent ancestor after ancestor till, at last, he represented the last of the race, the parent. The above view of heredity is necessary to my argument, and apparently is opposed to other and more modern

theories which at present seem to hold the field—for instance, Weismann's theory of Germinal Selection, or Mr Francis Galton's theory that so much of an individual is derived from this ancestor, so much from that, and so much more from a third. Every one of these latter theories ignores what seems to me the patent fact that the characters of all the ancestors are not commingled in the final result, the adult, but that during ontogeny each parent is represented in turn. It is true that watching the development of an individual, we cannot say that at such and such a point the great-grandparent ends and the grandparent begins, that at this other point the grandparent ends, and behold—the parent! The changes are too complex and subtle, too swift and fleeting; moreover, at every turn the variations from his ancestry of the individual under observation strike in and add to the apparent confusion.

It may be objected that the child during his development does not represent exactly, nor even closely, any of his remote ancestors; and this objection would appear fatal to the above theory of heredity. On the other hand any sufficient explanation of this vagueness of representation will go far to establish, not only this theory, but also that theory of retrogression which is the subject of this article, and which—if it be a true theory—is in a humble way the complement of the theory of evolution.

Offspring, as we know, vary from their parents, and if they vary, they must do so primarily in one of two ways. Either they must revert to the ancestral type, and resemble it more than the parent did, or else they must diverge from it still more than did the parent. The former variation we term 'atavistic,' the latter we may term 'evolutionary,' since it is on the lines of these latter variations that evolution proceeds. But of so-called atavistic variations, there are also two kinds; one of which is really atavistic or reversionary, whereas the other, though apparently atavistic, is actually evolutionary. True reversion occurs only when the individual varies so from his parent that, in his development, he does not recapitulate the whole of the life-history of his race, but stops short at a point reached by a more or less remote ancestor, whom in this way he resembles more than he does his parent. False atavism occurs when the individual, at an early stage of his existence, begins by recapitulating the whole of the life-history of his race up to his parent, but during a later stage retraces, or apparently retraces, some of the last steps made by himself in his development and by the race in its evolution, and thus, by a species of evolutionary variation, resembles a more or less remote ancestor more than he does the parent. Examples of this false kind of atavism are plentiful in nature.

The points here set forth are these. First, that development is a recapitulation of evolution, in other words, that every individual

repeats, though very rapidly and indistinctly, the life-history of his race, beginning with the unicellular organism and ending, in many cases, with the parent. Secondly, that an individual may so vary from his parent that he does not recapitulate the whole of the phylogeny, and that this constitutes true atavism, true reversion. Thirdly, that there is a false atavism, which is really evolution. This occurs when an individual after reaching the full development of his parent retraces some of the last steps of the ontogeny, and so resembles an ancestor more than he does his parent. More need not be said concerning the first proposition. As regards the third, it has been said above that examples of false atavism are frequent. From the nature of the case observation of it is difficult; for in every individual this retracement of the ontogeny, this false atavism, must be very slight—so slight as usually to be inappreciable. Therefore it is only by observing the retracement, not in an individual but in a line of individuals, that it becomes plainly noticeable. It is by taking advantage of such retracement that 'Reversed Selection,' as it has been termed, eliminates a structure which a change of environment has rendered not only useless, but worse than useless, more rapidly than would otherwise occur under the mere absence of selection. For example, Natural Selection has resulted in the evolution of eyes. In animals dwelling in absolute darkness, *e.g.* certain cave-dwellers, the eye has become not only useless, but worse than useless, since it is an extremely prominent and tender, and therefore vulnerable, part of the organism. In some such animals we observe that the eye is better developed in the embryo than in the adult. Clearly here the animal in its ontogeny retraces some of the steps it has already made. Clearly also, if ontogeny be a recapitulation of phylogeny, such retracement was made in the phylogeny as well. It follows that when a structure, useless both to the embryo and the adult, is better represented in the former than in the latter, it must have undergone retrogression through the action of reversed selection, and that during the phylogeny, after being useful, it became not only useless, but worse than useless.

The second proposition, that an individual may so vary from his parent as not to recapitulate the latter stages of the phylogeny, and that this constitutes true atavism, is the main proposition of the present thesis; but I have yet to prove that this atavism is the cause of true retrogression.

True atavism can seldom be observed in such of the higher animals and plants as have been evolved under Natural Selection, not because it does not occur, but simply because it is usually masked and slight. It is masked because such complex beings seldom or never retrogress in all their characters at once, and, therefore, such reversion as may occur in this or that particular is associated with

evolutionary variation in other particulars. It is slight because, since such species have evolved but slowly, reversion to a not very remote ancestor does not result in any appreciable change of type. Thus, under ordinary circumstances, if a man reverted in any particular to an ancestor of a thousand years ago, no one would recognise to what the change of type was due. Not only would the change be too slight, but the observer would need to have a knowledge of the ancestral form, and such knowledge is usually impossible. Sometimes, however, recognisable reversion does occur even among such beings. Thus a man may resemble the portrait of some far-away ancestor, or again the progeny of an ordinary pair of horses may exhibit the zebra-like stripes of a remote ancestor. It is not, however, among complex beings, slowly evolved in every particular, that we must seek our proofs. We must turn to plants and animals that have undergone swift evolution in some one particular, and this, so far as I know, occurs only under stringent Artificial Selection. For Natural Selection, having care for many characters, results in but slow evolution; but Artificial Selection, having care for only one or only a few characters, results in much swifter evolution. Supposing, then, we take any breed of domesticated animals or cultivated plants, and, after choosing the finest specimens, henceforward breed indiscriminately from these and their descendants; what then happens? It is notorious that under such circumstances cessation of selection is marked by a reversion towards the ancestral type, a reversion swift in proportion to the swiftness of the antecedent evolution. Thus, without continued stringent selection, the speed of race-horses cannot be maintained; they tend to lose their special characters, and revert to the ordinary horse. The same is true of all other prize breeds. Again, careful breeding from ordinary horses readily evolves a speedier race, for the offspring of ordinary horses in many instances surpass the parents. But, in proportion to the success of the breeder, further improvement grows continually more and more difficult, till at length evolution practically reaches a standstill. Improvement thereafter is very slow indeed. For this reason it is now very difficult to improve our breed of race-horses. The offspring of a pair of the finest animals are in the great majority of cases inferior to their parents, and, therefore, practically all that the most stringent selection is now able to achieve is to preserve, not to improve, the race. It is, therefore, plain that, owing to the increasing tendency towards reversion, rapid evolution quickly slows down, till, even in the presence of stringent selection, it practically ceases.

But perhaps the most striking proofs of the present theory are furnished by certain cultivated plants (for instance the apple), which are usually propagated by means of slips or suckers—that is, by detached portions of the individual. Practically speaking, the most

favourable individual of a species has been chosen and multiplied by means of slips, the rest of the species being eliminated; and in each new seminal generation the process has been repeated. Such plants, therefore, have been evolved by a tremendously severe process of selection, resulting in an evolution much more rapid than is possible among animals or annual plants. But now supposing we chose any one of these highly divergent varieties, and without using any selection, bred from seed alone, what again would happen? There is ample evidence leading us to believe that in the vast majority of instances the variety would swiftly (that is, in a very few generations) revert to something very like the wild stock from which it originally descended, but not to the wild stock precisely, for, no doubt, while the cultivated species was undergoing evolution in one direction, it was, under the changed conditions, undergoing retrogression in other particulars, and in these the reverted varieties would differ from the wild stock.

I need not dwell longer on the tendency such plants and animals have towards retrogression. The facts are notorious. But it seems to me that these facts are strongly adverse to all those recent theories of heredity to which I have alluded, and which suppose that each ancestor is not represented in turn during the ontogeny, but that the characters of all or many of the ancestors are commingled or are latent in the final result, the adult—Weismann's theory of germinal selection for instance, or Mr Galton's theory, which supposes that, on the average, one quarter of the total heritage of an individual is derived from the parent, one-eighth from the grandparent, one-sixteenth from the great grandparent, and so on. Were such theories true there could be no retrogression except through reversed selection, for the more evolved ancestors would for ever tend to make their influence felt. But plainly retrogression occurs in the mere absence of selection. Moreover, if it be true that the organic world has arisen through the preservation and accentuation of favourable variations, and if it also be true that ontogeny is a recapitulation of phylogeny, then it seems to me that it must be further true that there is necessarily a greater tendency towards retrogression than towards evolution. For all atavistic variations must tend towards retrogression; whereas all evolutionary variations need not constitute extensions of the previous evolution. They may result in divergencies in new directions; or may even constitute reversals of the previous evolution, as in those cases of which Reversed Selection takes advantage. Given sufficient time, in the absence of selection, retrogression must therefore necessarily ensue.

The rationale of retrogression, I take it, is as follows:—Suppose, as regards any character which has undergone evolution,

that A B C D represent a line of individuals; then if D reverts to B, that is if D varies from his parent C in such a way that in his ontogeny he represents the life-history of the race only up to the point reached by B, omitting the additional characteristic of C, it is evident from the point of view of heredity, that the series becomes A B D; or rather it becomes A B, since in effect, D is B. C then disappears completely and for ever from the series; and it follows that, if the characters of C ever reappear in E, or any subsequent member of the series, they must do so as a result of fresh evolution, not as a result of reversion. It is necessary to emphasize this point for on it my whole argument depends. If D, on the other hand, varies in such a manner from C that after representing C, that is after recapitulating the whole of the phylogeny he reverts back to B, then C does not disappear from the series. C will still be represented in the ontogeny, and, if his characteristics reappear in any individual at the end of the ontogeny, that is in the adult, it will be as a result, not of evolution, but of reversion. As I have already indicated, it is on such cases as the latter that Reversed Selection works. Thus, when during the phylogeny any character becomes useless and selection ceases, retrogression eliminates it with a speed which is proportionate to the speed of evolution. But, if it becomes worse than useless, then an additional factor steps in to hasten the elimination. Reversed Selection then takes advantage of such apparently atavistic, but really evolutionary variations as cause an individual, after he has represented his parent, to revert back again to a remoter ancestor. Moreover Reversed Selection not only preserves such individuals, but also eliminates all such individuals as have the worse than useless characters in a greater degree than their parent, and thus prevents them from influencing posterity.

It would be well to illustrate the foregoing with a concrete case. Suppose we plant seeds of those garden plants which I have instanced as having undergone very swift evolution. In a great number of cases the young plants revert towards the ancestral wild type. Now I have enquired everywhere, and I have never heard that the seeds of such a reverted plant, or of any of its descendants have ever reproduced the cultivated type. This means that the cultivated type has disappeared absolutely from the series. It will never again be represented in the ontogeny, and could reappear only as a consequence of fresh evolution, resulting from selection as stringent as that by which the cultivated type was originally evolved; if it did reappear without fresh evolution it would be because the reversion to the wild type had resulted not from true atavism, not from a lapsing of the last steps of the ontogeny, but from the false atavism on which Reversed Selection works. But,

since the retracement on which Reversed Selection works is apparently always small in amount, it never seems to occur in species that have been so rapidly evolved as these garden plants. Their reversion, therefore, seems to be invariably due to true atavism, there being apparently no room for Reversed Selection. Here, then, is a strong proof, convincing proof as it seems to me, that true atavism means a lapsing for good and all of the last steps made in the phylogeny.

Two things are evident from the foregoing. First, that there is on the average a greater tendency towards reversion than towards evolution, that is, there is a greater tendency to revert towards the ancestry than away from it, in other words, there is a greater tendency to let lapse in the ontogeny the last steps made in the phylogeny than to add other steps to them. Secondly, the strength of the tendency towards reversion is proportionate to the swiftness of the antecedent evolution, and, therefore, species which have been quickly evolved, tend to retrogress swiftly, whereas species, which have been slowly evolved, tend to retrogress slowly. For this reason it is that characters long established in the species are much more stable than more recent characters, for, in the former case, reversion, to be appreciable, must be to an extremely remote ancestor, whereas in the latter, reversion to a much less remote ancestor results in appreciable retrogression.

Suppose now a certain character in a line of individuals has undergone evolution. Denote by the symbols A B C D E F, the evolution of the character in successive individuals of the line, A being the rudimentary character as it appeared in the first of the line who had it, F the character when it reached its highest perfection. Suppose that cessation of selection occurs as regards this character. Then F tends to be lapsed, and, when it is lapsed, E reappears at the end of the ontogeny. But thereafter E also tends to be lapsed, and D to reappear, and so on, till, in the continued absence of selection, at length A reappears. But under the same law A tends likewise to disappear, and then the character vanishes utterly, and the race reverts to that ancestral condition when the character did not exist. In this manner I take it do useless parts disappear absolutely. Thus have disappeared, for instance, the limbs of the snake. Thus have disappeared the eyes of some cave-dwelling animals, and the many useless parts of parasites. Thus have vanished innumerable useless parts in every plant and animal.

We are now in a position to consider the part played by reversion in nature. Every complex individual, as we know, varies in a thousand ways, great and small from its parent; but only here and there is a variation useful. The useful variations, in proportion to their usefulness, are preserved and, in succeeding generations, are

accentuated by Natural Selection. The useless variations, the vast majority, are planed away by reversion. Most of them being minute, disappear in the next generation, but, even when they are comparatively great, a very few generations suffice to procure their disappearance. Even should a series of individuals happen to vary in such a manner that in each successive individual a useless character is more and more accentuated, yet, since the tendency towards atavism is greater than towards evolution, a time surely comes when, perhaps in a single generation, the whole of the evolutionary variations lapse and the character vanishes, never to reappear, except in the improbable event of fresh evolution of a like nature. Again it sometimes happens that a change of environment renders useless a structure which was formerly useful. Here also reversion steps in and procures its elimination. Such a structure—say the wing of a bird, the habits of which have ceased to be aërial—was evolved by the superimposition in a long line of individuals of favourable variation on favourable variation. These, when the character becomes useless, are lapsed in orderly succession, the most recent first, the more ancient later; till, at last, the structure reverts to that most ancient condition when it did not exist. In this manner it approximates continually to more and more ancient forms, but only approximates. It never reproduces its prototypes of the phylogeny exactly, for during the whole course of evolution, reversion was at work, planing away everything which was originally useless, or which became useless as the environment changed. A complex organ such as a wing is, therefore, a product not only of evolution but also of reversion. Evolution roughhews the organ, but reversion chisels its finer lines. What is true of a complex organ is true in a yet greater degree of every complex plant and animal. Such a being is a product not only of evolution, but also of reversion. In it many structures, useful during a remote period of the phylogeny, but useless later, have disappeared utterly by reversion to that yet more ancient condition when they had not come into existence. Others, in which reversion is yet incomplete, still persist, and are known to us as vestigial remains. It should, however, be noted that, when a vestigial structure is more developed earlier in the ontogeny than it is later, this indicates that its retrogression is due not only to reversion the result of true atavism, but to false reversion the result of Reversed Selection. Such a structure must have become not merely useless, but worse than useless during the phylogeny.

Every complex animal, therefore, in the successive stages of its development does not represent exactly successive stages in the evolution of its race. At each stage of the ontogeny are present useless structures, or useless parts of structures, which have retro-

gressed backwards towards a more ancient order of things; and at every stage of the ontogeny structures are absent, which were present in the phylogeny because they were then useful, but which since underwent complete retrogression, because they subsequently became useless. Here, then, we have the explanation of the fact that ontogeny is only a very vague recapitulation of phylogeny. Doubtless if a higher animal, a man for instance, lived during his ontogeny in a succession of environments similar to those in which his race was evolved, his ontogeny would much more exactly recapitulate the phylogeny than it actually does, for in that case structures, which had been useful during the phylogeny, would continue to be so during the ontogeny, and so would be preserved. But consider how vastly different is the environment, in which the embryo of man develops, from the environments in which his race evolved. The embryo develops in the uterus, but its free prototypes struggled each for itself in a world full of enemies, full of eliminating agencies. How many parts, therefore, have become useless to the embryo, which were useful to the prototypes! How vast is the field in which retrogression has worked! Is it any wonder, then, that the ontogeny of man is only a vague recapitulation of his phylogeny?

Reversion, then, is the necessary complement of evolution, and without it there could be no evolution, except of the simplest kind. Without reversion there could be no planing away of the numberless useless variations which occur during, and especially at the end of the ontogeny, nor of all those structures, which, though useful during some part of the phylogeny, became useless later. Without reversion, therefore, a species would soon become so burdened with useless variations and structures as to be incapable of existence. Reversed Selection could not cause the elimination of all these useless and burdensome characters; for no matter how burdensome, and, therefore, worse than useless, they are in the aggregate, separately they are so little burdensome that Reversed Selection could not act. It could not act on them in the aggregate, for this would mean that in some individuals they would be present *en masse*, whereas they would be absent *en masse* in others; and this, of course, we know is not the case. Moreover Reversed Selection causes a retracement, not a lapsing of characters. It therefore works at a double disadvantage as compared with ordinary Natural Selection, and, as a consequence, can effect comparatively little. No extensive examples of such retracement are in fact known to us in Nature. Again, without retrogression, the recapitulation of the phylogeny in the ontogeny would be impossible, and, for this reason once again, evolution would be impossible. For, were there no retrogression, the prototypes of the phylogeny would necessarily be

reproduced exactly in the ontogeny, and then the latter would be as elaborate, and almost as lengthy as regards time, as the former. Moreover, the prototypes of the phylogeny could not exist in the enormously changed environment of the ontogeny. How, for instance, could a gill-breathing animal, or any of the higher forms which intervene between them and man, exist in the uterus, in which alone can exist those dim representations of the phylogeny that constitute man's ontogeny?

It is this great change of environment, this close protection of the individual in the uterus and afterwards, which has rendered possible the evolution of man and the other higher animals. Opportunity has thus been afforded for retrogression to plane away innumerable characters which had become useless. The ontogeny has thereby been straightened, shortened, and simplified, and the evolution of new characters, useful in the new environment, has become possible. Thus, for instance, have been rendered possible the higher characters of man, for even after birth he is closely protected, and, therefore, even in that portion of the ontogeny which intervenes between the infant and the adult has there been much retrogression. Consider how feeble and helpless is the infant after birth; but its prototypes of the phylogeny fought for their own existences. The infant can digest scarcely anything but milk, and its jaws are very feeble. Its prototype must have had much wider powers of digestion. Perhaps more remarkable than anything else is the retrogression of instinct in man. I have dealt at length with this question elsewhere, and have not space for it here; but consider how helpless is the infant at birth, how extremely incapable, as compared to young insects, for instance, of adapting itself, of its own initiative, to the environment. Later on it acquires all kinds of knowledge and ways of thinking and acting, which serve as a superior substitute for instinct. But meanwhile the mother's protection, which has rendered possible this acquirement, has rendered useless also the instincts of its prototypes, which have therefore lapsed. Hence the retrogression of instinct in man. By it his mental ontogeny is shortened and simplified, just as by the retrogression of bodily parts his physical ontogeny is shortened.

In the foregoing I have spoken of characters lapsing in orderly succession, the last first, the earlier later. But it seems to me probable that earlier characters may sometimes lapse before the later. This may happen when some parts of the phylogeny, and consequently of the ontogeny, are not direct, but form a loop, so to speak. The omission of the loop would straighten, and therefore shorten, the ontogeny, and considering how condensed is the latter, I believe this must often occur.

G. Archdall Reid.

9 Victoria Road South, Southsea.

V

The Movement of Diatoms

THIS paper was suggested by a perusal of Robert Lauterborn's "Untersuchungen über Bau, Kernteilung und Bewegung des Diatomeen." It would be impossible to do justice to a work in which the letterpress occupies 165 quarto pages, within the limits of a single article. The present communication, therefore, will deal mainly with the movement of Diatoms, omitting, for the present, any reference to the interesting chapters on the protoplasm and its inclusions, and on nuclear and cell division—a portion of the work well worth separate treatment. For ready comprehension, however, the subject must be introduced by an account of the structure of the frustule in *Pinnularia* and *Surirella*.

Structure of the Frustule in Pinnularia major.—The general appearance of *Pinnularia* is sufficiently well known to render any special description superfluous. The chief features visible in a surface view of the frustule, as shown in Plate III., Fig. 7, are (1) the presence of a median longitudinal undulating line, the 'raphe,' with terminal and central nodes, and (2) a double row of transverse markings, the so-called 'striae' or 'costae.' The true significance of these structures is brought out clearly by Lauterborn.

The raphe had previously been interpreted by the majority of investigators as an open cleft placing the interior of the cell in direct communication with the surrounding medium, and Pfitzer also sought to justify this view on various grounds. Flögel, however, contended that the cleft was closed on its inner side by a thin membrane. In order to settle this point Lauterborn carefully examined empty frustules of *Pinnularia major*, and also transverse sections (2-3 μ) of examples fixed with chromosmium and stained with haematoxylin. One of these sections, from the region between the central mass of protoplasm and the extremity of the cell, is shown in Fig. 1. The cell-wall exhibits a marked thickening in the neighbourhood of the raphe, which runs as a narrow angular cleft from exterior to interior without any distinguishable trace of an inner limiting membrane. The appearance of this cleft varies considerably according to the position of the section in the series (see Figs. 2 and 3), being at some points simply oblique, at others variously bent, and this irregularity in shape explains why the aspect of the raphe alters with a change of focus. In some sections (Fig. 3),

the walls of the cleft approach one another very closely in such a manner as to simulate the appearance of an internal closure. In those examples in which the cleft, seen in transverse section, has an angular course, O. Müller considers it probable that the central portion is closed during life by the middle lamina, in which case two canals would persist, one running along the outer surface, the other along the inner surface of the cell-wall, but Lauterborn has not observed such a closure, nor does he consider its occurrence probable. This is of some importance, since Müller postulated the presence of such canals in his theory of the movements of diatoms.

As to the transverse markings (Riefen) of the frustule, Lauterborn's results are entirely in agreement with those of Flögel, who had previously described the appearances as being, in reality, chambers hollowed out in the substance of the frustule, and communicating by a tolerably wide opening with the interior of the cell. These features are well shown in Fig. 1, where the plane of section passes through four of these chambers, and Fig. 2, which is an outline drawing of a section passing between two chambers in one valve of the frustule. These drawings also illustrate the way in which the two valves are united by the overlapping of their free edges.

The interior of the cell is lined during life by a layer of protoplasm, thinner laterally than elsewhere, and, in the middle of the cell, forming a transverse bridge-like mass containing the nucleus. On each side of this central portion the peripheral protoplasm surrounds a large vacuole filled with cell-sap. This condition of things suggested to Lauterborn a possible explanation of its meaning, which, although purely hypothetical, is worth consideration.

The researches of O. Müller have shown that the protoplasm of *Pinnularia* is subjected to a very considerable osmotic pressure (4 to 5 atmospheres), manifested with equal intensity in all directions. Now pressure in the direction of the lateral walls would result in pressing the overlapping elements closer together, but when exerted at right angles to this, in the direction of the raphe, there would be a tendency to force the two halves of the frustule apart, if the frictional resistance of the overlapping elements was not sufficient to maintain equilibrium. But, as we have already seen, on each side of the raphe internally a great number of transverse chambers are placed one behind the other, and filled by prolongations of the peripheral protoplasm that enter each chamber by an opening about half the diameter of its internal cavity. Lauterborn suggests that these chambers filled with abstricted portions of the protoplasm might be imagined to act as so many clamps (' Klammern ') opposing a strong resistance to the force tending to separate the two halves of the frustule, and so strengthening the frictional resistance of the overlapping elements. He points out

that this hypothesis would gain in probability if it could be proved that the protoplasm was withdrawn from the chambers when the two halves of the frustule parted from one another during cell-division, a circumstance which he regards as by no means improbable. Pfitzer does not consider the hypothesis summarised above very probable, but believes that the chambers with their very thin outer walls would probably relieve the osmotic pressure; Lauterborn, however, seems justified in his contention that the two functions in no way exclude one another.

Structure of the Frustule in Surirella calcarata.—This diatom is one of the largest and most beautiful fresh-water forms. Examples measuring 0·3 mm. in length and 0·2 mm. in lateral breadth were not uncommon, so that, under favourable conditions, they could be readily perceived with the naked eye.

Seen in transverse section (Fig. 4) the outline is roughly rectangular, with two sides ('Gurtelseiten') flattened, and the other two ('Schalenseiten') more or less hollowed out on each side of a median ridge or keel. At each corner is an outstanding process (*ala*, 'Flügel'). These processes vary in appearance with the plane of section (see description of Plate) and three aspects are shown in the figure, combining features exhibited by different sections. The drawing also illustrates the complicated character of the chromatophores, and their relation to the central bridge-like mass of protoplasm containing the nucleus. When viewed in its entirety, and with one of the lateral surfaces (flattened sides in section) turned towards the observer, the diatom presents the appearance of a more or less broad wedge, the median portion occupied by the lobed chromatophores (superficial focus) and flanked on each side by the *alae* which are now seen to consist of a number of parallel transverse canals connected at their extremities by a longitudinal canal, and separated from each other by tolerably wide interspaces. In a surface view ('Schalenseite') the contour of the frustule is almost lanceolate with a median longitudinal ridge produced at either end into a spur-like process, the anterior one being the largest.

To return to the alae, Figure 5 shows a portion of one of these wing-like processes viewed from the surface. Along the edge, for its whole extent, passes a rather narrow canal ('Flügelrandkanal' or 'Flügellängskanal') a connection being established between this and the interior of the cell by numerous short transverse canals. Internally, these latter commence as tolerably wide tubes of rounded section; towards the longitudinal canal (*lc.*), however, they steadily diminish becoming more elliptical in section. The transverse canals are separated from one another by U-shaped intermediate pieces, formed of apposed portions of the cell-wall, whilst the spots where

they remain separate give rise to the canals. Consequently, each of these intermediate pieces forms a niche-like depression between two projecting transverse canals and a corresponding piece of the longitudinal canal. Lobular processes of the chromatophores project into the transverse canals and are always enveloped by protoplasm, which passes from their extremities towards the outer wall as a strand, usually undivided. These strands exhibit both longitudinal and transverse fibrillation during life, and, as a rule, appear to consist of five or six longitudinal rows of cellular compartments.

The longitudinal canal was described by Flögel as closed externally, but Lauterborn's sections show that it is interrupted along its outer edge by a very narrow cleft, thus placing the interior of the cell in direct communication with the surrounding medium (Fig. 4). This cleft is not only visible in sections through the frustule, but can also be made out in a surface view when one of the alae is viewed vertically, parallel to the direction of the transverse canals. Yet another circumstance, not taken into account by Flögel, points to a breach of continuity in the cell-wall at this spot. In the living *Surirella*, foreign bodies (*e.g.* particles of Indian ink, sand, small diatoms,) are readily observable adhering along the edges of the alae, where they are moved briskly to and fro in the same manner as along the raphe of *Pinnularia*, *Navicula*, etc., a circumstance very difficult to explain if the cell-wall were in reality unbroken.

Most of the *Surirella* were infested externally by a small alga belonging to the Cyanophyceae (*Chroococcus* sp.). The small blue-green spherical cells always occurred in a definite situation, viz., in the niche-like intermediate pieces of the alae, only a single alga being present, as a rule, in each cavity (Fig. 5 *pa.*). More than once, examples of *Surirella* were noticed where all the intermediate pieces of the alae sheltered these little lodgers. It is pointed out that the advantage may well lie exclusively on the side of the *Chroococcus*, a possible explanation being that the alga, seated on the diatom in the very fine and easily disturbed mud, suffers less interruption of the assimilative processes than if it were free in the mud, because the diatoms, if buried by the movements of fishes or creeping molluscs, soon work their way up again to the surface and so to the light. In this connection it may be mentioned that Gastrotricha, of the genera *Chaetonotus* and *Ichthydium*, constantly attach their eggs to the surface of large *Surirellae*.

The Movement of Diatoms.—Various hypotheses have been advanced to account for the characteristic movements more or less familiar to every student of the Diatomaceae. Amongst earlier investigators, M. Schultze held that protoplasm, protruded through the slit-like raphe, served to set the cell in motion, and this view was maintained by Pfitzer and Engelmann. On the other hand Nägeli,

Siebold, Dippel, Borscow, and particularly Mereschkowsky, regarded the cell movement as the result of an osmotic phenomenon, supposing water to be imbibed at the anterior end of the diatom and expelled with greater force at the hinder extremity, the recoil serving to propel the cell onwards.

Bütschli and Lauterborn worked together at the subject, and the former published a preliminary account of their researches, which was adversely criticised by O. Müller. A reply from Lauterborn drew a further communication from Müller, and this is dealt with in the work under consideration, where the Bütschli-Lauterborn observations and inferences are given at length, with a detailed criticism of Müller's objections and theory. The observations described and illustrated by Lauterborn first claim attention, and will be best given as nearly as possible in his own words.

When large examples of *Pinnularia* are brought into a concentrated emulsion of Indian ink,[1] the majority of the diatoms present at the first glance a very striking appearance, each being surrounded by a broad, bright, and sharply defined border, within which the smaller granules of Indian ink do not penetrate. In a surface view this halo usually follows the contour of the frustule at a distance equal to about half the width of the valve; but when the lateral aspect of the diatom is turned towards the observer, this clear border is seen to be interrupted in a symmetrical manner at both ends of the cell, and also in the vicinity of both central nodes (Fig. 8); here the granules of Indian ink approach close to the cell-wall.

The appearance just described is interpreted by Lauterborn as pointing to the existence of an enveloping layer of hyaline jelly, so remarkably transparent, and possessing a refractive index corresponding so closely with that of the surrounding water, as to be completely invisible in clear water, even when examined with the best lenses.[2] Müller denied the general presence of a gelatinous envelope in the sense advocated by Bütschli and Lauterborn, stating that the clear border only appeared after a long sojourn in the Indian ink emulsion, and that it was absent in the living but completely motionless cell. He, however, subsequently admitted the existence of a gelatinous envelope. In answer to Müller's objections on this point, Lauterborn asserts that the hyaline border becomes visible as soon as the diatoms are brought into the emulsion, and also states that he has often seen *Pinnulariae*, surrounded by the transparent envelope, remain for hours without the slightest movement.

The presence of a peculiar and characteristic streaming move-

[1] Lauterborn recommends that the Indian ink should be rubbed up in the water in which the diatoms have been cultivated, and also that specimens from old cultures should not be employed in studying movement.

[2] A gelatinous envelope of similar transparency has been observed in a pelagic form of *Cyclotella comta*.

ment in *Pinnularia* is next described, and will be found illustrated in figs. 6 and 7. It can be seen to greatest advantage in a lateral view of the diatom in Indian ink emulsion (fig. 6). Small granules of Indian ink, set in motion in the region of the anterior[1] terminal nodes, move outwards, and then flow, at some distance from the surface of the cell, as far as the central node of the raphe. The granules borne along by these anterior streams (*gs.*) are not closely packed, and exhibit a more or less active independent movement amongst themselves. Arrived at the middle of the cell the streams of granules turn sharply towards the openings of the central nodes, forming there larger or smaller accumulations, and from these points arise granular threads (*gt.*) which always run obliquely backwards, forming an acute angle with the surface of the frustule. In watching the formation of these granular filaments, Lauterborn observes that the process strongly suggests the forcible expulsion, by fits and starts, from the central node, of a gelatinous thread, to which a single row of granules adheres. This view is strengthened by the circumstance that the granules at the moment of union with the thread at once lose their active molecular movement perceptible whilst within range of the anterior streams of granules, also that the granules are all moved backwards intermittently with the same velocity, as the thread lengthens, suggesting the presence of a common substratum binding all of them together. It often happens that the series of granules is interrupted at some point, nevertheless the direction and intensity of motion in the granules behind such a gap correspond with those in front of it, and this circumstance appears to offer additional evidence of the presence of a common bond in the shape of a gelatinous thread, absolutely invisible in clear water until brought into view by the adherence to it of foreign bodies. These threads, which lengthen as the diatom moves onward, may be five or six times as long as the frustule.

The phenomena described above are best observed when the diatom is lying on its side, as in fig. 6 ; but in a surface view (fig. 7) the streams of granules along the raphe on both sides of the frustule can be demonstrated by altering the focus. Occasionally the granule stream and thread on one side of a diatom move in the opposite direction to those on the other side, so that the two streams neutralise one another to a certain extent, and sometimes only one granular thread is developed.

In a paper published in 1891 Bütschli considered it highly probable that a causal connection existed between the backward prolongation of the granule threads and the local movements of the diatom, stating that both he and Lauterborn were disposed to

[1] The terms 'anterior' and 'posterior' are here used with reference to the direction of movement of the diatom.

regard these movements as due to a copious production of adhesive jelly ejected rapidly and with a certain force from the nodes of the frustule. Whether the extremity of the gelatinous thread struck the substratum on which the diatom rested or only encountered the resistance of the water, the effect would be the same, viz., to drive the cell in the opposite direction.

In opposition to this view O. Müller explained the phenomena as caused by a stream of cytoplasm propelled through the anterior terminal nodes into the external cleft of the raphe (which, it will be remembered, he supposed to be closed by a median lamina), and there moved towards the centre, flowing back into the interior of the cell through the canal of the central node. The stream, projecting laterally from the cleft, swept with it the suspended granules in the neighbouring layer of water, bearing them towards the narrow central canal. Here a congestion and accumulation of the cytoplasm would occur and, as the effect of the stream on the granules ceased, the latter would collect more or less, become agglutinated by the dammed-up protoplasm, and subsequently displaced backwards. So, according to Müller, the thread originated, and as the cytoplasmic stream moved intermittently, the thread would likewise elongate intermittently, and appear as if ejected from the canal of the central node. The essential feature of Müller's hypothesis lies in the view that the material extruded from the interior of the cell is true protoplasm; a view he still maintains in a later communication noticed in a postscript to Lauterborn's work.

Lauterborn argues at great length against the validity of such a conclusion, pointing out first of all that the invisibility of the extruded material under ordinary conditions militates against the protoplasmic theory, since streaming protoplasm is always recognisable as such without difficulty. It is true that Müller cited Schultze in support of the contention that under certain circumstances the presence of true plasma-streams might escape detection, but in the special cases mentioned by Schultze (*Gromia*, *Difflugia*) the contour of the pseudopodia was always clearly defined. Further, it is necessary to remember that protoplasm, which when Schultze wrote (1865) might be described as 'hyaline' or 'structureless,' is so no longer, thanks to improved optical appliances, and there is no ground for supposing that, in cases where granules are moved along by the pseudopodia of rhizopods, the protoplasm ever resembles the streaming substance in *Pinnularia*. Lauterborn also lays stress upon the fact that the collections of cytoplasm normally occurring at the poles of the *Pinnularia* cell exhibit a beautifully defined reticular structure during life, and that it is in these regions that Müller's streams of cytoplasm originate. To imagine that the cytoplasm is completely changed in passing through the polar cleft

to the exterior, so that no structural feature can then be detected even by the best apochromatic objectives, would seem highly improbable to say the least. The formation of the thread-like prolongations originating at the central node would, on Müller's hypothesis, appear to involve a considerable waste of living substance during prolonged movements ;[1] this could scarcely be brought into harmony with the economy of a single cell, and no proof is given in support of his assertion that the main streams of cytoplasm return to the interior of the cell by way of the canal of the central node. On the whole, according to Lauterborn, everything tends to show that the main streams (*gs.*) consist throughout of jelly which is driven out at the ends of the cell through the openings of the terminal nodes (especially the 'crescentic polar clefts' of Müller), moves in the raphe towards the centre, and projects laterally over it to some extent. The fact that larger fragments of Indian ink or granules of carmine sunk in this jelly are carried in it along the raphe towards the centre proves that the entire hyaline border is actually in motion throughout, but the precise mode of formation of the gelatinous threads and their relation to the rest of the streaming substance is acknowledged to be obscure.

In contrast to the conditions existing in some species of the genus *Pinnularia*, there are other diatoms, *e.g. Pinnularia oblonga* and members of the genera *Navicula*, *Pleurosigma*, and *Nitzschia*, in which a gelatinous envelope and thread-like prolongations are apparently wanting. In these, the small grains of Indian ink come into contact with the siliceous frustule, and are often moved actively about close to the raphe, as described by numerous observers. So far, the most careful examination has failed to show anything projecting from the raphe, and it would therefore seem that the substance which causes the movements of the foreign bodies, moves within the fissures of the cell-wall, whether occurring in the alae of *Surirella*, the keel of *Nitzschia*, or elsewhere. Lauterborn goes at length into the question whether a substance streaming within a narrow cleft, and so touching the surrounding medium only with a narrow linear portion of its surface, could effect the locomotion of the entire cell, and concludes that a sufficiently powerful force might be developed to overcome the frictional resistance of the surrounding water, pointing out that a somewhat similar principle (the so-called 'hydraulic reaction') has been successfully employed to propel large ships.

Owing to the impossibility of examining the substance presumed to be streaming in the raphe, one cannot say whether it is protoplasm or a gelatinous material, and it must be conceded that the

[1] Müller has since suggested that these prolongations may consist of granules only (smoke-streak appearance), but a connecting substance seems necessary in order to explain certain features observed by Lauterborn.

same mechanical effect would be produced in either case. Lauterborn is content to describe it as a viscous substance, though he apparently inclines to the belief that it corresponds to the jelly already described in *Pinnularia*, and believes that corroborative evidence for this view is furnished by an examination of *Surirella*. The structure of the alae in *S. calcarata*, with the arrangement of the longitudinal and transverse canals have been already described. The displacement of foreign bodies along the clefts in the longitudinal canals has been long known, and it is pointed out that, although the protoplasmic strands surrounding the chromatophores in the transverse canals exhibit, in the living diatom, a well-marked reticular structure, the contents of the longitudinal canals are perfectly hyaline and can only be brought into view by staining. Thionin and methyl violet were used, after which the substance in question appeared as a granular contracted string, from which the plasma of the transverse canals was generally separated.

With reference to the capability of diatoms to produce considerable masses of jelly under certain circumstances, it is pointed out that there are a considerable number of forms in which single cells are united into colonies of variable shape by gelatinous material (*Encyonema*, *Schizonema*, *Mastogloia*, etc.). In other cases the cells are borne upon long gelatinous stalks, as in *Achnanthes* and *Gomphonema*, whilst, in auxospore formation, numerous diatoms, generally free from jelly, are known to secrete it very profusely. In places where diatoms are massed together, covering the mud on which they rest with a brown scum, it is only necessary to take a piece of the latter between the fingers in order to appreciate its slimy nature, due to the production of jelly by the living cells. A movement by the aid of a gelatinous material produced by the living cell is not confined to the Diatomaceae, but is found in other forms, both animal and vegetable. Desmids, *e.g. Closterium*, secrete a gelatinous thread by the help of which the cells are able to raise themselves upon the glass walls of the culture vessels. In the form mentioned, the thread emerges through pores at the extremity of the cell, and can only be demonstrated by staining, or by the employment of an emulsion of Indian ink.

Again, in the case of *Oscillaria*, Lauterborn describes and figures the adhesion of foreign bodies to, and their movement in spiral paths along, the algal threads. As the *Oscillaria* moves forwards, a bright streak appears at the hinder end (visible in Indian ink, or by staining) and lengthens as the *Oscillaria* advances; it is apparently to be regarded as consisting of a jelly-like substance separated on the surface of the algal thread and drawn backwards in a spiral manner. A very similar mode of progression is also met with in the Gregarines; these, according to Schewiakoff, also produce a long glutinous track,

which can be brought into view by the methods alluded to above. His illustrations show how great is the resemblance to the corresponding phenomena in the Desmidieae and *Oscillaria*.

In concluding the part of his work dealing with the movement of diatoms, Lauterborn briefly criticises a theory advanced by Hauptfleisch in 1895. In opposition to other observers Hauptfleisch would locate the organ of locomotion in *Pinnularia* in protoplasmic threads issuing from pores on the longitudinal edges of the diatom. These pores, however, are apparently non-existent, and both Lauterborn and Müller agree in considering the protoplasmic threads to be merely contracted portions of the plasmatic cell-body occupying the inner chambers of the frustule.[1]

Methods of Collecting and Preserving Diatoms.—In collecting, a spoon attached to a stick was employed for skimming the brown diatomaceous ooze off the surface of the mud, whilst in the case of forms occurring at greater depths, *e.g. Surirella*, a small drag-net was found useful for bringing samples of mud to the surface. This latter was placed with water in shallow glass vessels sheltered from direct sunlight, and after resting for about twelve hours the diatoms appeared in masses on the surface of the mud, whence they were readily transferred by means of a pipette to the fixing-fluid.

Among fixing reagents, Flemming's chromo-aceto-osmic acid, and sublimate in either water or alcohol solutions, demonstrated the most delicate structural features of the nucleus and cytoplasm during division. Picro-sulphuric acid followed by a haematoxylin stain gave excellent pictures of the chromatic elements of the nucleus. A 1% osmic acid solution served, in unstained preparations, to bring out the arrangement of the cytoplasm, the chromatophores, and other inclusions in the cell. A 45% solution of iodic alcohol is recommended for the study of the so-called 'red granules' of Bütschli, which stain exceptionally well after fixing by this method.

Only large forms could be removed individually under the dissecting microscope and by means of a capillary tube; the smallest forms were taken up in the mass by a pipette and at once placed in a tube containing the fixing solution. Here they remained for about fifteen minutes, after which, the fixing fluid being decanted off, they were well washed in water, and afterwards passed, through alcohols of increasing strength, into absolute alcohol, where they remained until all the colouring matter of the chromatophores was extracted, and any oil globules removed. The addition of a drop or two of sulphuric ether and the application of a moderate amount of heat facilitated this process. Afterwards, the material was passed through alcohols of decreasing strength into distilled water, in readiness for staining.

The most useful stain was a weak solution of Delafield's haematoxylin, but it was necessary to control the process under the microscope in order to prevent overstaining. Alum- and borax-carmine were also tried, but with decidedly inferior results. Safranin was useful

[1] O. Müller, in a paper which I have not yet read (*Ber. Deutsch. Bot. Gesell.*, xv. (1897), pp. 70-78), seems now to regard movement as taking place by means of currents of a mucilaginous substance projecting from the raphe. See *Jour. Roy. Micro. Soc.*, 1897, p. 234.

for demonstrating the centrosome and nucleoli in specially prepared material. When stained, the specimens were passed successively through 35%, 70%, 95%, and absolute alcohol into oil of cloves for clearing purposes, and finally mounted in dammar. The alcohol baths must be changed very gradually both when fixing and staining, otherwise distortions of the protoplasm are certain to occur. Isolation of the nucleus in *Surirella* could be accomplished by placing the stained specimen in dammar under a cover-glass provided with wax feet and pressing this down until the frustule began to gape. Then, by gently and persistently tapping the cover-glass with a needle, the nucleus could often be completely freed for examination.

It was possible to stain the diatoms to a certain extent during life in a very weak solution of methylene blue (1 in 100,000), in which they would live for days. If transferred from this to a stronger solution (0·01%), some of the cell contents were stained in a very characteristic manner; but so soon as the nucleus began to take up the stain, it was a sure sign that the vitality of the cell was on the wane, although the diatom might continue to move slowly for a time. In no case was observation of living specimens omitted, for a whole succession of phenomena could only be adequately studied in this way, and such observations possess special value for checking the results obtained after the use of reagents. A Seibert apochromatic objective of 2 mm. focal length was usually employed, in combination with a No. 12 ocular, giving a magnification of about 1200.

Town Museum, Leicester. F. R. Rowley.

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SOME NEW BOOKS

The Hypnotising of Animals

Beiträge zur Physiologie des Centralnervensystems I. Die sogenannte Hypnose der Thieren. By Max Verworn. 8vo, pp. iv+92, with 18 text-figures. Jena: G. Fischer, 1898. Price M. 2.50.

Professor Verworn is to be congratulated upon the production of the above work. The subject-matter is one of very great interest; it is set forth in such lucid and agreeable style as to make the book excellent reading, whilst the method of treatment adopted by the author gives the treatise a high value, and renders it an experimental contribution to the physiology of the nervous system based upon original lines. This will be made clear by a short sketch of the scope and aim of the work.

The so-called 'hypnosis' of animals is a well-known state of immobility resembling on superficial examination the condition of hypnotic trance which can be produced in man. A dissimilarity between the 'state' in lower animals and that in the hypnotised human subject is however present at the very outset, the means of production being different in the two cases. In man those mental states which are implied by the term 'suggestion' play an important part as precursors of the condition; but in lower animals the essential agency is the maintenance of the body by external force in an abnormal position. Examples are given in the book of the state of immobility into which animals of very different types may fall under these conditions. The original part of the work is the demonstration by Professor Verworn that the immobile state in these animals has two prominent characteristics which are significant of the physiological factors concerned in its production. These are, first a special form of activity in the muscles, and, secondly, a peculiar condition of inactivity of the cerebral hemispheres.

With regard to the muscles the author shows that persistent reflex tonus is present, and is most marked in such groups of muscles as the animal would utilise for regaining its normal position of bodily equilibrium. If, for example, the state has been caused in the guinea-pig by keeping it upon its back, then the muscles concerned are those the animal employs for turning into the customary attitude. When held by force in the abnormal position the animal vainly contracts these muscles for this purpose, and the immobile state commences with the sudden cessation of the nervous outflow from the higher centres producing these vain efforts; this is immediately succeeded by a set tonus of the muscular groups especially those involved in the previous efforts. If the restraint has been such as to affect one group more than others, then the subsequent tonicity or 'contracture' is particularly prominent in this group. The state thus differs from sleep in the disposition of the limbs, trunk, head, eyes, &c., which may be made to

assume all manner of different appearances by appropriate previous manipulation, so that the attitude can be of a most bizarre kind. During the continuance of the immobile state the organs of sensation, peripheral and central, show no evidence of any alteration, and the animal thus appears to be conscious of the various sensations produced by external impressions, but if these are sufficiently intense to evoke an efferent discharge from the cerebral hemispheres, the state at once ends and recovery takes place. This recovery is shown to be ushered in by augmented contractions of those muscular groups which are in the state of more pronounced contracture; at first these are ineffectual to alter the position of the whole body, but with their repetition the contracture subsides and the alteration is effected. The author has not been able to confirm Danilewsky's observation that during the state reflex excitability is lowered; he regards the previous evidence of such lowering as due to the peculiar condition of the muscles, which owing to contracture are incapable of adequate response to central nervous discharge. Lowered reflex excitability only occurs when an animal has been manipulated many times in rapid succession, in which case central fatigue manifests itself and the contracture is correspondingly diminished. It will be seen from the foregoing description that the characteristic condition of the muscles, *i.e.* tonicity, disproves the existence of any inhibition of lower neuro-muscular mechanisms during the state; on the contrary, the lower centres—cerebellum, medulla, &c., being released from cerebral control, now discharge a continuous stream of nervous impulses such as occurs in the decerebrate mammal, and this produces the marked decerebrate rigidity described by Sherrington, Horsley, and others.

With regard to the second factor in the production of the state, it is shown that the cessation of the discharge of impulses from the cerebral cortex is a complete one. Such complete cessation must exist when the cerebral hemispheres have been previously removed; in such animals the immobile state can be produced with great ease, and is always of a most prolonged type, whilst the stimulating agencies necessary to produce recovery, have to be of an intense character. In the intact animal the author considers that the sudden cessation of cerebral discharge cannot be explained as due to the lack of stimulation of the motor areas existing in the cerebral cortex since the physiological avenues for sensation are unaffected. He believes that the paralysis of these centres is brought about by a sudden inhibition due to the activity of special parts of the nervous system or to special conditions of the centres themselves. His conception of such conditions is set forth at some length in the concluding chapters of the work, and is framed upon the lines of Hering's well-known views of the physiological states of activity and repose.

The whole work affords most striking instances of the opposite rôles to be assigned to cerebral and to lower centres respectively, hence, its perusal may be confidently recommended to all those whose special interests lie in physiology or neurology. But apart from the obvious scientific value of the book, the earlier chapters contain a description of phenomena which will enable those interested in hypnotism to realise what the so-called hypnotic state of an animal is like, and

how it can be produced. A further interest is given by the excellent account of the observations and views of other writers upon the subject, from Pater Kircher who described the *experimentum mirabile* in 1646 down to the present day, and the value of this is enhanced by the well-selected woodcuts with which the account is illustrated. The whole forms a work which fully sustains the well-merited reputation Professor Verworn has derived from his previous publications.

FRANCIS GOTCH.

MR BEDDARD ON BIRDS

THE STRUCTURE AND CLASSIFICATION OF BIRDS. By Frank E. Beddard. 8vo, pp. xx + 548. London: Longmans, Green & Co. 1898. Price 21s.

THIS volume comes to us as a promise long delayed, and conjures up visions of men whom many of us have never seen, but who yet live in their works, and will live, as long as ornitholgy, in its deepest and truest sense, continues to be studied amongst us. It represents three occupants of the Prosectorial Chair of the Zoological Society of London. It was begun by Garrod, and contemplated by Forbes, but before either could come within a measureable distance of its completion they were summoned by death, and another entered into their labours. It was for Mr Beddard, the present Prosector, to realise what they had always hoped to do, and than him a more fitting person could not be found. Perhaps the highest praise we can give will be to say that the result is worthy of all three, and that it would have met with the entire approval of either of his predecessors.

Although the work of Garrod and Forbes has been largely drawn upon, Mr Beddard has incorporated the essence of all that is best of his own work and that of his contemporaries.

He divides his book into two parts—(1) General Structure; (2) Classification. Under the first head, among other subjects, he deals with the coelom, convolutions of the intestines, and the syrinx in a full and able manner, and these pages will be found to contain a vast amount of most valuable matter. It surprises us, however, to find that no figure is given of the passerine syrinx.

The possession of feathers, an ambiens muscle, and an oil-gland are the characters enumerated by Mr Beddard as peculiarly avian, the two last having been acquired within the class. About the first and last of these no one has ever expressed any doubt, but there are many who have regarded the ambiens as reptilian in origin. The absence of an oil-gland, it is pointed out, may be a primitive and not pseudo-primitive character. The Struthiones are defined as birds in which the gland is absent, though a page or two further on it is stated to be present in *Apteryx*, and correctly so. The primitive feathering of birds Mr Beddard thinks was in the form of downs. "The persistence of downs, therefore, in this hypothesis is so far a primitive character, and the greater the persistence the more primitive the bird." We certainly doubt whether this is not proving too much. If this is true, then the Anseres, Accipitres, Charadrii, and Ralli, for example, would be more primitive than the Pico-passeres, or the Struthiones and Galli. Again "the fact that the contour feathers are frequently preceded by downs" is not convincing proof that this was the primitive covering of birds. Inasmuch as the down-feathers

which precede the contour-feathers are quite distinct from those which form the down-feathers proper. The former in the ducks, the fowls, megapodes, and tinamous, for instance, probably much more nearly represent the primitive clothing, and are in the nature of semiplumae, which as Garrod and the present writer have shown, are degenerate contour-feathers. We incline to agree with Gadow and to hold that the absence of down-feathers is primitive.

The presence of teeth is undoubtedly primitive. "Arrested dental papillae" are instanced as occurring in *Phytotoma rara* and in the merganser. The present writer has figured and drawn attention to similar structures in the tinamou and *Opisthocomus*.

In describing the skull, no mention is made of the parasphenoid. We are told that the "base of the brain-case is protected by a large basitemporal which has sometimes (*e.g.* Apteryx) a long rostrum in front." For sometimes 'always' should surely be substituted. The description of the hyoid is unintelligible. As regards the pelvis, we entirely agree with Mr Beddard in favouring the view that the pelvis of Aves most nearly resembles that of the dinosaurs. We further agree with him in regarding the pectineal process as the equivalent of the forwardly directed process of the dinosaurian pubis, and the backward process as the homologue of the pubis proper in the two groups.

The classification of birds is a subject of peculiar difficulty, and no two ornithologists can be persuaded to think alike on this matter. Its importance in Mr Beddard's estimation can be gathered from the fact that he has devoted two-thirds of his book to this question. And in these pages will be found some extremely valuable and helpful suggestions, which will afford food for reflection for a long time to come. He divides the class into two sub-classes, 'Ornithurae' and 'Saururae,' corresponding to the Neornithes and Archaeornithes of Gadow. The Ornithurae are further divided into 'Anomalogonatae' and 'Homalogonatae.' But it is unfortunate that nowhere is the latter group defined or are its boundaries fixed. As to the arrangement of the sub-orders we will only say that we should have preferred to see the Tubinares placed next the Sphenisci, the Tinami near the Galli, and the Accipitres nearer the Steganopodes and Herodiones.

Space forbids discussion of this book at greater length. Some room must be left wherein to protest against any suspicion of captious criticism or querulous fault-finding. Whatever statements we have taken exception to have been selected not as an instance of many such, but as blemishes to be removed should a second edition be called for, which is highly probable. Those who have occasion to use this book most will learn soonest to find out its sterling value, and such will best appreciate the fairness of our remarks. W. P. P.

"O THOU WONDROUS MOTHER-AGE!"

THE WONDERFUL CENTURY: ITS SUCCESSES AND ITS FAILURES. By Alfred Russel Wallace. 8vo, pp. xii+400, with 12 folding tables and frontispiece portrait. London: Sonnenschein. 1898. Price 7s. 6d.

THIS book is an appreciation of the nineteenth century, an attempt to look at it in its relations to the whole history of man as it will appear

to the historian of the future. The successes have been in an increased knowledge of the facts and governing principles of the world around us, and in the application of them to our benefit. The failures have lain chiefly in the field of social economy, in which the advance has been incommensurate with that in the region of physics.

The striking feature of the century has been the discovery and application of scientific and mechanical principles entirely unknown to previous ages; discoveries comparable to the invention of fire, of writing, of geometry, or of printing; applications that have revolutionised the mode of life of nearly all the world, bringing changes both wide and deep where change had been unknown for centuries, or even for millennia. Chief among these are the means of communication by railways, steamships, the electric telegraph, and the telephone. Then come modes of lighting, friction matches, gas light, and electric light. The knowledge of light itself, and its action on matter, with the marvellous applications to photography, the Röntgen rays and spectrum analysis, by which last our knowledge of the distant universe has been so enormously extended in so many directions. Minor mechanical inventions of a novel order are the phonograph, the typewriter, and the cycle. Among scientific theories, whose practical application, though not always so direct or obvious, has profoundly altered our ways of thought, or given us fresh mastery over matter, Mr Wallace notes the following:—The doctrine of the conservation of energy; the molecular theory of gases; the atomic theory as the foundation of modern chemistry; the uses of dust; a knowledge of meteors and the meteoritic theory of the universe (the latter perhaps not so generally accepted as to have a right to rank in the present category); the hypothesis of a glacial epoch (in which also Mr Wallace goes further than many admit); the vaster conception of the antiquity of man; the cell theory and the theory of recapitulation in embryology (where, likewise, a hint of recent criticism would not have been misplaced); the germ-theory of disease and the function of leucocytes, from which conceptions Mr Wallace, not quite fairly, separates antiseptic surgery; the use of anaesthetics; and the acceptance of the theory of organic evolution, an acceptance due chiefly to the labours of Darwin, whose "work will always be considered as one of the greatest, if not the very greatest, of the scientific achievements of the nineteenth century."

This first half, or rather, less than half, of the book is a well-balanced and thoroughly interesting review, making its chief appeal to the ordinary intelligent reader. It might have been written, perhaps not quite so well, by any competent man of science. The second section of the book, dealing with the failures of the century, could have been written only by Mr Wallace. As an expression of the convictions of an eminent naturalist and thinker on many of the most important problems of our day, it has a value by virtue of that personal element, and demands the attention of all, whether they agree with its opinions or no. The list opens curiously with a strong statement of the case for phrenology, the neglect of which is regarded as one of the chief failures; the chapter undoubtedly provokes one to a reconsideration of the subject. Of similar nature is the opposition to hypnotism and physical research, so prevalent among scientific men.

It is true that exposure of charlatan after charlatan has raised a serious prejudice against such truth as does lie in these obscure branches of knowledge, while the sensational appeal they make to the unbalanced mind of the uneducated must always lead the scientific investigator to approach them with a caution and scepticism greater even than that which he rightly applies to all subjects of his study. But to write this down as a failure is to go too far. Much has been attempted and accomplished by trained observers and professional medical men, while mention at least should have been made of the establishment of the entirely new science of psychology, from the critical and experimental study of which far more promising results have already been derived than from the not always edifying exhibitions of mediums and clairvoyants. To judge from the fact that a quarter of the book is devoted to it, vaccination is the subject on which Mr Wallace feels most strongly. The opponents of this operation can hardly say that it was accepted and enforced with unthinking speed; moreover, improvements have been and are constantly being made, and we can hardly regard the statistics here collected as applicable to the vaccination of the future. The concluding chapters deal with militarism, the treatment of criminals, concentration of capital with its corresponding increase of absolute poverty, followed by the deterioration of those brought under its influence, and the spoliation of the products of the earth, such as forests, coal, and the fertile soil. That in these respects our century is no better, and often far worse than its predecessors, is too generally admitted to need emphasis here. But, whether or no the remedies to be adopted are those suggested by Mr Wallace, we venture to believe that remedies are being sought for most earnestly by an increasing number of men and women, and that even the nineteenth century may claim more than is here allowed to it. Arbitration has made progress, the treatment of criminals has improved, co-operation has become more general, schools of forestry are held to be essential, even the much-abused Indian Government has constructed irrigation works that will be the wonder of ages to come, and, as Sir William Crookes lately told us, the chemist is prepared to refertilise our worn-out soil.

Let us not be too pessimistic. No advantage is ever gained without a corresponding disadvantage, and we cannot look for advance in all directions at once. The evils that we all deplore have been caused by those very benefits that we give thanks for, and a recognition of the evil is the first step towards its removal. As an honest attempt to look things straight in the face, Mr Wallace's book deserves a welcome from men of all opinions.

Geology made Easy

Geology for Beginners. By W. W. Watts. 8vo, pp. xviii+352, with 310 illustrations. London: Macmillan & Co. 1898. Price 2s. 6d.

This is one of the best introductions to Geology that we have ever seen. Most books of the kind are ship-wrecked on a syllabus; but Mr Watts, though planning his work on the lines of the revised syllabus of the Science and Art Department, has managed to steer safely through its narrow passages without sacrificing breadth of view or originality of treatment.

Two features of the book are prominent. One, for which the author expresses indebtedness to the late Prof. Green, is a constant appeal to actual observation and experiment, the value of which in so practical a science as geology can hardly be overrated. The other, which is indeed the natural corollary of the former, is the introduction of numerous photographs of actual sections and views of geological interest. For this Mr Watts has peculiar facilities, as secretary to the British Association Committee for collecting and preserving such photographs. On the whole these photographs are well selected, and add greatly to the attractiveness of the book. But nearly all of them suffer from the printing, and there are many which we have found in actual use to be unintelligible to the student. Figures 13 and 19, purporting to show Crinoidal and Wenlock Limestone, might be almost anything, so great is the reduction. Figures 18, 60, 88, 104, 154, 155, 296, 297, 301, are among those that would have been more effective as pen-and-ink line-drawings, such as the excellent fig. 53. The introduction of cross-country sections is to be commended; but the compression of a section across the Snowdon range or the ancient rocks of Pembrokeshire into three inches does not make its unravelment either easy or pleasurable. We rejoice to see that the very clear woodcuts of De la Beche are still available. As for the figures borrowed from Zittel and distinguished by (Z), Mr Watts doubtless knows who the true authors of most of them were and has suppressed the intelligence after due consideration. We allude to this because there is a lamentable tendency on the part of text-book writers to copy figures from other text-books, and to give credit to the copyist or compiler rather than to the original author. Thus the student is led in a mazy round and not to the fountain-head.

In style and arrangement the work is remarkably clear. General or doubtful statements receive their necessary qualification, so often omitted in elementary text-books. Errors there may be, but they are not very serious. One fault should be remedied in a future edition; that is the introduction of technical terms in the legends to figures, without any explanation in the text, or without cross-reference to such explanation if given on a later page. Ice-tables, for instance, should either have been explained, or they should not have had a half-page illustration devoted to them.

The book is a fitting celebration of Mr Watts' appointment to an assistant-professorship at Mason College, Birmingham. On its appearance, both the author and the beginner in geology are to be congratulated.

The Affinities of Animals

L'anatomie comparée des Animaux Basée sur L'embryologie. Par Louis Roule. 8vo, pp. xxvi+1972, with 1202 figures. Paris: Masson et Cie. 1898. Price 48 francs.

Mr Roule is Professor at the University of Toulouse and is well known by his works "Embryologie générale" and "Embryologie comparée," to which the two bulky tomes before us form a natural sequel. The object of the book is not to give either a systematic summary of the whole animal kingdom, a detailed account of its numerous variations of structure, or even an elaborate discussion of

selected types, but rather to show the relationships of animals and thus to manifest evolution in the Animal Kingdom. As the chief guide in determining those relationships, Prof. Roule takes embryology, although palaeontology and comparative morphology and histology are not rejected.

The Animal Kingdom is divided into sixteen branches, dealt with in the following order: Sarcodic Protozoa, Ciliate Protozoa, Mesozoa, Spongida, Hydrozoa, Scyphozoa, Plathelminthes, Nemathelminthes, Trochozoa (Rotifera; Bryozoa, Brachiopoda, Phoronidea, Sipunculidea; Mollusca; Archiannelida, Hirudinea, Chaetopoda; Pseudannelida = Sternaspids & Echiurians), Arthropoda, Chaetognatha, Peripatida, Echinoderma, Enteropneusta, Tunicata, Vertebrata. Each of these branches is dealt with under the following heads: General considerations and relations to other branches; Distribution in nature; General organisation, first of the embryo, then of the adult; Comparative account of the different body systems, as manifested in the various Classes of the Branch; Principles of classification, division into Classes, and mutual relations of the Classes; Bibliography. The second volume ends with two indices, the first to the zoological names, the second to the anatomical terms. There is no general chapter dealing with the Animal Kingdom as a whole, or with the classification adopted, since that was given in the previous works referred to above.

Seeing that the subject is one of such obscurity, and open to so wide diversity of opinion, it hardly seems worth while pointing out the paths along which we should not care to follow our professorial guide. That he has made the attempt, and that Messrs Masson have published it, is alone a reason for gratitude. For the book, though somewhat wordy, and occasionally less clear than we are accustomed to from a Frenchman, furnishes a series of very readable accounts with many suggestions of interest. Without casting any slur on embryological research, we must confess to some distrust in those who place quite as much reliance on it as does Prof. Roule. But taking it at his valuation, we fail to see how it lends support to the view that the Nautiloidea are ancestral to the Ammonoidea and the admittedly dibranchiate forms; for their embryology shows clearly that the Nautiloidea have lost an important structure, the protoconch, once possessed by them and still possessed by the other orders. We also venture to think that the known facts in the embryology of recent echinoderms afford no proof whatever that the five-rayed ancestor, which the holothurians must have had in common with the other classes, was less developed than many cystids. Prof. Roule's 'hypothetical Pentazoön' is not the most ancestral form that is shadowed forth to us, either by embryology or by palaeontology.

Special praise is due to the illustrations, which have nearly all been drawn for the work, under Prof. Roule's direction, by Mr L. Jammes, in a style that is at once original and effective. We must, however, protest against the picture on p. 1275, purporting to represent living crinoids, "dans un fond rocheux de convention" (penny peep-show convention). Attached to one of these marvellous rocks, by a stem far too short in proportion to its arms, is a *Pentacrinus*; how it is fixed one cannot tell, but certainly not by the cirri, as

undoubtedly it would be in life. Below it, on a bottom that is apparently muddy, is a *Holopus*, which certainly ought to have been attached to the rocks, though not on the same page as the *Pentacrinus*, of which genus there appear to be other specimens, wildly waving about on a vertical precipice in the background. Fortunately, this is the only picture of the kind. The printing of the book is excellent, but the type and paper used have made the volumes rather too portly for comfort. With the works of the two Perriers, of Delage and Hérouard, of Blanchard with his corps of specialists, and of Roule, our friends across the Channel suffer from no lack of home-made text-books. And on this they are distinctly to be congratulated.

Plant Life

Plant Life Considered with Special Reference to Form and Function. By Charles Reid Barnes, Professor of Plant Physiology in Chicago University. 12mo, pp. x + 428, with 415 text-figures. Holt: New York, 1898. Price $1.12.

We have nothing but praise for this excellent introduction to the study of plants. The author describes it as an attempt to exhibit the variety and progressive complexity of the vegetative body; to discuss the more important functions; to explain the unity of plan in both the structure and action of the reproductive organs; and finally to give an outline of the more striking ways in which plants adapt themselves to the world about them. It is meant to supplement genuine and regular work in the laboratory. There are four parts: Part I. The vegetative body, traces the increase in morphological differentiation from the unicellular organism to the seed-plant, and then discusses the general structure of root, shoot, stem, and leaves. As regards the terms primary and secondary, we note that the former is used to express the original root developed from the egg, the latter being applied to adventitious roots wherever developed. Part II. Physiology, deals with the general facts of the physiology of the individual, namely, maintenance of form, nutrition, growth, and movement. Reproduction is treated separately, under the headings vegetative and sexual, in Part III. Part IV. Ecology, contains chapters dealing with forms of vegetation in relation to different sets of conditions; Mesophytes, or the ordinary land plants with which dwellers in fertile temperate climates are acquainted; Xerophytes, or plants adapted to dry conditions; Hydrophytes, or those adapted to a more or less aquatic life. This part also comprises chapters on symbiosis, the relations of plants to animals, and the protection and distribution of spores and seeds. There are several useful appendices, including directions for a course of laboratory work, and for collecting and preserving material, with lists of apparatus, reagents, and reference books. An important feature of the volume is the great number of excellent figures, with an unusually full explanation in each case.

The Physiological Study of Plants

Practical Plant Physiology: an Introduction to original research for students and teachers of Science, Medicine, Agriculture, and Forestry, By Dr W. Detmer. Translated from the second German edition by S. A. Moor. 8vo, pp. xx + 556, with 184 illustrations. London: Sonnenschein & Co. 1898. Price 12s.

Yet another botanical hand-book translated from the German! The translator's desire in undertaking this work is to promote the teaching

of plant physiology in England, where he thinks it is seriously retarded by the lack of suitable books. He admits the excellence of Francis Darwin's "Practical Physiology of Plants," but seems to think that an English work on more advanced and comprehensive lines is needed.

Prof. Detmer's "Praktikum" covers nearly the whole field of experimental plant physiology in the widest sense, ranging from the rheotropism of Myxomycetes to the breaking stress of bast fibres, and from unpalatability with snails to emulsion figures simulating protoplasm. Yet the whole does not strike one as being, in the highest sense, a book—an expression of a personality—as does the "Experimental Physiologie" of Sachs, or even the "Praktikum" of Prof. Strasburger; but it has rather markedly the air of being pieced together. It is indeed an encyclopaedia of methods, which have been carefully overhauled—an immense piece of work—by the author. It seems as if it ought to be very valuable as a work of reference, yet one is not quite sure what class of student will refer to it. It is no doubt really intended for the small number of advanced students who are about to undertake research on the physiology of plants, and so gives an account of the stock methods of investigation.

The experiments are grouped, in logical sequence, into five sections, viz., the Food of Plants, the Molecular Forces in Plants, the Metabolic Processes in the Plant, Movements of Growth, and Movements of Irritation. The successive experiments are not categorically limited, but are linked together by theoretical and expository paragraphs, so that the book can be read continuously; but this involves so much additional space that in many experiments small details have to be omitted, the neglect of which will prevent the experiment being successful on first trial.

The second German edition (1895) is nearly one-third longer than the first edition (1888), has been largely re-written, and contains a short appendix on recent views on the ascent of water.

The weakest section in the present edition is that on the application of the polariscope. It is said to be 'very instructive' to investigate the phenomena exhibited in polarised light by starch grains (which, however, should be mounted in Canada balsam for this purpose, not in water); but surely, beyond the pretty effects, the student learns nothing from his observation but the fact that starch grains are anisotropic. This may be due to a remote crystalline structure or to internal tensions. The latter hypothesis is not mentioned though it might easily have been illustrated experimentally. As a further obscurity, 'gypsplättchen' has been translated in the English edition as 'plates of gypsum' (p. 115). This should of course be plates of selenite, which have special optical properties.

The most complete sections are those on Respiration, which have been considerably expanded, and to which Prof. Detmer and his pupils have contributed original work.

The English edition is translated without alteration or addition from the second German one. There is something to be said for retaining unchanged the appendix, which gives an annotated list of the German dealers in, or makers of, the scientific apparatus described in the book; but a patriotic editor might have indicated where some of the articles could be obtained in this country. It is not necessary

to send to Germany for klinostats and polar planimeters any more than for platinum crucibles and india-rubber tubing.

The translator has done his work excellently, and we are but rarely reminded of the German original. We note, however, a few errors. The English of 'plasmolytische gemacht' is 'plasmolysed,' not 'rendered plasmolytic' (p. 145). On p. 43, l. 19, 'layer' would be intelligible while 'meniscus' is not, though the German is 'Meniscus.' On p. 424, l. 16, 'favourable' is found where 'unfavourable' is meant.

The abundant matter of this compendious book is conveniently arranged, and the translation of it will be welcomed by the proportionally increasing number of those among even serious botanical students, who are unable to use such a work in the original.

The Organism as Unit

Problems of Biology. By George Sandeman. 8vo, pp. viii+213. London: Swan Sonnenschein. 1896.

The Living Organism: An Introduction to the Problems of Biology. By Alfred Earl. 8vo, pp. xiv+272. London: Macmillan & Co. 1898. Price 6s.

These two books appear to have similar aims and to arrive at much the same ultimate conclusion, or absence of conclusion, and yet they stand in strong contrast to each other. Mr Earl's book is nicely printed and bound; it is written in an easy style, has all the clearness that can be imparted by division into chapters with many subdivisions and running headlines, uses italics where appropriate, and is furnished with a full table of contents and with an index. Mr Sandeman's book is needlessly repellent in type and binding; its style has an individuality that is strongly marked, but far from attractive or lucid; the chapters and the paragraphs are of wearying length; there is no analysis of contents, and no index. Nevertheless, we set Mr Sandeman's book above Mr Earl's: it is more interesting, more critical, and more suggestive. If the author would rewrite it with more feeling for the dulness of his readers, if he would be guided by the example of Mr Earl, and if he would temper his biological erudition to the ignorance of the philosopher, his philosophical jargon to the simplicity of the biologist, we should recommend both parties to read his second edition. We dare not recommend the perusal of the first, except to those superior beings who have mastered "Sordello" and "Bygmester Solness."

For both of these writers a theory of the unity of the organism forms the chief object of biological enquiry. Each of them is at pains to tell us in what this unity does not consist; but, as is natural, neither of them can formulate a clear conception of what it is. Mr Earl lays stress on the impossibility of conceiving the organism apart from its environment; they may be expressed in terms of subject and object, and constitute "a dual manifestation of a single reality." Mr Sandeman dallies with 'feeling' as that which gives unity to the organism, but presently rejects it and falls back on the barren conception of 'character,' which, he says, "is the identity in difference

of concrete individuals, and is the familiar expression for the whole of a system," that is to say, a pure abstraction. To us the most fruitful conception appears to be that of 'memory'; in other words, the tendency to repeat the same action or process under similar, or almost similar, conditions. What, if any, may be the ultimate physical cause that causes a given readjustment of molecules in any one mass of protoplasm to be repeated rather than replaced by another adjustment, we do not know. But if this tendency, not unknown in the inorganic world, be admitted for protoplasm, then at least we have a phenomenal foundation for theories of specific segregation, individuality, and heredity.

Mr Sandeman's critical artillery is levelled at the three postulates of biology:— (1) "that the qualities of the individual are separate constituent elements of which the organism is the total sum"; (2) that "all the qualities of the organism and all its stages are the manifestation of, and are related to, one another only through an agent or system of agents within the known body"; (3) that "everything organic exists only by reason of, and is to be explained only in relation to, some special external use which it now has, or which a similar structure has had in former times." The demolition, from a philosophical standpoint, of various biological theories is well worth reading by practical naturalists. Mr Earl's book should also be read by them, for it is to be feared that in our modern schools of science hardly enough attention is paid to the logic and fundamental conceptions of the subject. "The dissection of typical organisms is not necessarily an intellectual exercise." F. A. B.

Diet and Blood

Natural Hygiene; or, Healthy Blood the essential condition of Good Health, and how to attain it. By H. Lahmann, M.D. Translated by Dr H. Büttner. 8vo, pp. viii+254. 5 plates. London: Swan Sonnenschein & Co. 1898. Price 4s. 6d.

Dr Lahmann is a vegetarian and an enthusiast; his book has passed through many editions in Germany, and has been translated into other tongues; now for the first time it appears in English. The author is able to show a considerable amount of scientific reason for the faith that is in him, which may be epitomised in three propositions:—(1) that ordinary diet is deficient quantitatively and qualitatively in the mineral salts required by the human body; (2) that we consume far too much sodium chloride; and (3) that we take in water to excess. The truth of at least the first two of these propositions is incontrovertible, and Dr Lahmann deserves credit for calling attention to them. On this basis he builds his doctrine of "dietetic dysaemia," and proceeds to expound a new pathology for most of the ills that flesh is heir to—from short sight to difficult labour. In our opinion, he falls into the error of pushing his doctrine to unwarrantable extremes—the fate of most enthusiasts. No intelligent person can fail to admit the force of certain of his contentions, and no sane pathologist can repress a smile at others. The book is worth reading, and is likely to do much more good than harm.

Introductions to Chemistry

Chemistry for Schools: an Introduction to the Practical Study of Chemistry. By C. Haughton Gill. Tenth Edition. Revised and enlarged by D. Hamilton Jackson. Crown 8vo, pp. x+356, with 105 figs. London: Stanford. 1898. Price 4s. 6d.

The Organized Science Series. First Stage. Inorganic Chemistry (Practical). By F. Beddow. 8vo, pp. viii+166, 37 figs. London: Clive. 1898. Price 1s.

Of the making of elementary text-books of chemistry there appears to be no end. We have recently received copies of the above works, and although it scarcely enters into our province to review them critically, we may say that they both appear to be very clearly written, and to be well adapted to the requirements of the student. Neither of them differs very startlingly from others of the kind. Perhaps of the two the smaller book shows more originality of treatment. We are glad to see that, even in so elementary a book, at least thirty pages are devoted to quantitative experiments. In "Chemistry for Schools," the five-page chapter on Crystalline Systems is quite inadequate, and in parts unintelligible. It is time that text-books of chemistry contained really clear and detailed expositions of at least the elementary principles of crystallography.

L'Année Biologique.

We welcome the second volume of this excellent 'Biological Record,' even though it be issued some twenty months after the last of the publications with which it professes to deal. In regard to accuracy of quotation and comprehensiveness, it is an improvement on the first volume, noticed in *Natural Science* for August 1897. As we said before, absolute completeness is hardly to be hoped for, and certainly is not attained by Professor Delage and his collaborators. For instance, although most of the appropriate papers that appeared in our own pages during 1896 are indexed, we see no reference to Miss Newbigin's valuable contribution on the pigments of animals; or does this not come under 'Biology'? However, there is a list (in itself useful) of nearly 900 periodicals said to have been consulted in the preparation of the volume. The abstracts, so far as we have checked them, seem done with intelligence and accuracy; critical remarks are, as a rule, inserted between square brackets. At the beginning of each subject an attempt is made to give a general view of advance in that field, and in certain cases this has led to the publication of elaborate essays. Such are that on phagocytosis in the animal kingdom, by J. Cantacuzène, with preface by E. Metchnikoff, and that on marine zoogeography, by G. Pruvot. The publishers are Schleicher Frères, 15 Rue des Saints-Pères, Paris, and the price is 20 francs.

Varia

We have received from Messrs Friedländer & Son, of Berlin, a copy of *Naturae Novitates* for 1897. This valuable record is issued in parts twice a month; and finally indexed, bound, and sold for four marks, at the middle of the succeeding year. It is a record of all books that appear dealing with Natural History and the exact Sciences, and is invaluable in its fortnightly form for ready reference, as well as in

its annual form for general reference. *Naturae Novitates* provides moreover a regular and important collection of Personalia as regards appointments and deaths.

THE second part of vol. i. of the *Records of the Botanical Survey of India* contains a paper on the results of Lieut. E. Pottinger's journey through Myitkyina, Burmah. There is a complete list of the phanerogams and vascular cryptograms and a map. Many orchids are recorded.

THE re-issue of Stanford's "Compendium of Geography" will be completed in 1899. Vol. i., by Geo. C. Chisholm, is complete, and contains the birth of the mainland of Europe. Vol. ii. will include the British Isles, Scandinavia, Denmark, and the Low Countries, and is already advanced. Other volumes, to contain Central and South America, are in the hands of Sir Clements Markham and A. H. Keane.

THE *Annual Progress Report* of the Geological Survey of Western Australia for 1897 has just reached us, in the contorted and unmanageable condition usual with these official publications. The report consists of 66 pages and no less than seven valuable maps (Northampton, Peak Hills, Horseshoe Diggings, Bunbury, Kanowna, Coolgardie, and Artesian Bores in the vicinity of Perth), all of which have accompanying text.

OUR interesting contemporary *The Journal of School Geography* includes among the varied contents of its October number an illustrated article by G. K. Gilbert on the "Origin of the Physical Features of the United States," reprinted from the *National Geographic Magazine* for July. There is also a useful note by A. J. Herbertson, defining the various terms Britain, Great Britain, England, &c.

FURTHER LITERATURE RECEIVED.

Aids in Practical Geology, Cole: Griffin, London. Text-book of Botany, Vines: Sonnenschein, London. Text-book of Mineralogy, Dana: Wiley, New York. Cytologie expérimentale, Labbé: Carre & Naud, Paris. Allgemeine Biologie, I., Protoplasma, Kassowitz: Perles, Wien. Pflanzen-geographie, Schimper: Fischer, Jena. From Matter to Man, Dewar: Chapman & Hall, London.

Studies from Yale Psychol. Lab., vol. v. Report Australian Museum, 1897. Cornwall County Council, Report Fisheries. Study of the development of the Tuatara, Denby: *Proc. Roy. Soc.* Relations between marine animal and vegetable life, Vernon: *Mitt. Zool. Stat. Neapel.* Birds from Sierra Nevada de Sta Marta, Bangs: *Proc. Biol. Soc., Washington.* Ruins of Xkichmook, Thompson: *Field Columbian Mus. Publ.*, 28. Half-century of Evolution, Packard: *Proc. Amer. Assoc.* Report on Chillagoe mining district, Jack: Geol. Surv., Queensland. Relations between Hybrid and Parent forms of Echinoid. Larvae, Vernon: *Phil. Trans.*

Amer. Geol., Jan., Feb., March, Nov.; Amer. Journ. Sci., Oct.; Amer. month. Micr. Journ., Sept., Oct.; Amer. Nat., Oct.; Bolletino del Naturalista, 10; Botan. Gazette, Oct.; Feuille des jeunes Nat., Nov.; Irish Nat., Nov., Oct.; Scot., Geogr. Mag., Nov.; Timehri, XII., 1; Trans. Perthshire Soc., II., 6; Victorian Nat., Sept. and Oct.; Westminster Rev., Nov.; Journ. Conchol., Oct. (vol. ix., No. 4; Journ. School. Geogr., Oct., Nov.; Knowledge, Nov.; Literary Digest, Oct., 8, 15, 22, 29, Nov. 5; Naturae Novit, Oct., No. 18, 19, 20; Naturaleza (Mexico) (2) II., III.; Naturalist, Nov.; Nature, Oct. 20, 27, Nov. 3, 10, 17; Nature Notes, Nov.; Naturen, Oct.-Nov.; Proc. R. Soc. Victoria, XI., 1; Plant World, Oct.; Psychol. Rev., Nov.; Review of Reviews, Oct., Nov.; Revue Scient., Oct. 22, 29, Nov. 5, 12, 19; Riv. Ital. Sc. Nat., Nov.-Dec.; Science, Oct. 7, 21, 28, Nov. 4, 11; Scientific Amer., Oct. 8, 15, 22, 29, Nov. 5, 12; Scot. Med. and Surg. Journ., Nov.

OBITUARIES

James Spencer, the geologist and palaeobotanist, died on 9th July at Akroydon, Yorkshire. He was born 27th April 1834 at Luddenden, and for a time worked in a brickyard. He was "discovered" by Colonel Akroyd about 1853, and by him given a post as porter in his warehouse, from which position he speedily rose to that of cashier, remaining in Colonel Akroyd's firm until he retired in 1886. He was connected with the Haley Hill Literary and Scientific Society, the Halifax Scientific Society, and the Ovenden Naturalists' Society in many ways, and did a great deal to spread a knowledge of geology among his fellows. Spencer became actively associated with the late Professor Williamson about 1878, and was an important helper in the "Fossil Plants of the Coal Measures." A full account of his life and work appears, by Mr W. B. Crump, in the *Halifax Naturalist* for October.

Dr John Edward Tierney Aitchison, the well-known botanist and explorer, died on September 30, at Kew, aged sixty-three years. He was born in India in 1835, took his degrees in medicine and surgery at Edinburgh and entered the Bengal medical service in 1858, retiring in 1888. He paid especial attention to the botany of India, publishing his first paper in 1863 on the plants of the Jhelum district, with notes of considerable economic value. In 1869 he issued a catalogue of the flora of the Punjab and Sindh, and in 1878 he accompanied Roberts' expedition to the Kuram, acting as botanist. He made immense collections, which were worked out at Kew, the results appearing in the Linnean Society's publication. He obtained not less than 15,000 specimens from the Thal and Peiwarkotal districts in one expedition, and 10,000 on another occasion when he acted as naturalist to the Afghan Delimitation Commission. His labours led him over almost the whole of North-west India, Afghanistan, Baluchistan, Persia, and Russian Turkestan, and he was a botanist who had a keen idea of other things than mere dried plants. He has left much material which we hope will be worked out. We are indebted for these notes to a sympathetic article, by Mr Botting Hemsley, in *Nature*.

Luigi Lombardini, Professor of the Anatomy of Domestic Vertebrates at the Veterinary School of Pisa, died at that place on 27th June, having been born at Poggibonsi on 11th April 1831.

The deaths have also been announced of:—W. G. Atherstone, who worked and wrote on the geology of South Africa between 1856 and 1874; on 1st September, at Hobart, Tasmania, the well-known Australian conchologist, C. E. Beddome; on 5th August, Prof. Eugenio Bettoni, director of the fishery station at Brescia, aged 53; J. Crocq, professor of pathology in the University of Brussels; Giuseppe Gibelli, professor of botany and director of the Botanical Garden at Turin, on 16th September; on 25th September, Dr A. Lasard, at Nizza, aged 74; Don Francisco Coello de Portugal, president of the Geographical Society of Madrid; Heinrich Theodor Richter, lately director of the School of Mines at Freiburg; Michele Stefano de Rossi, the seismologist; on 31st May, at Wilmington, Delaware, U.S.A., the botanist, Edward Tatnall, aged 80.

NEWS

The following appointments have recently been made :—Miss Catherine A. Raisin, D.Sc., the well-known petrologist, to be Vice-Principal of Bedford College for Women, London ; C. B. Crampton, M.B. of the University of Edinburgh, to be assistant-keeper in the geological department of the Manchester Museum, in succession to H. Bolton ; Prof. C. Chun to be professor of zoology at Leipzig ; Prof. W. Kükenthal to be professor of zoology at Breslau ; Dr Conrad Keller of the Polytechnicum, Zürich, to be full professor of zoology ; Dr H. E. Ziegler, of Freiburg, i/B, to be professor of phylogeny at Jena ; Dr C. A. Kofoid to be assistant-professor of zoology at the University of Illinois ; Wallace Craig to be assistant in the State Laboratory of Natural History at the Illinois Biological Station ; E. B. Forbes to be field-entomologist of the Illinois State Laboratory of Natural History ; J. H. M'Gregor to be assistant in zoology at Columbia University, New York ; G. M. Holman as assistant in biology at the Massachusetts Institute of Technology ; Dr B. Moore, of University College Hospital, to be professor of physiology in the Yale Medical School ; Dr Albert Matthews to be assistant-professor of physiology at Tufts College ; Dr Simon Flexner to be professor of pathological anatomy at Johns Hopkins University ; Dr Joseph Priestly to be teacher of hygiene in the British Institute of Preventive Medicine ; Dr J. P. Hylan to be assistant professor of psychology at the University of Illinois ; Albert Gaillard to be director of the Lloyd Herbarium at Angers ; C. W. Young to be assistant in botany at the University of Illinois ; James Pollock, Hamilton Timberlake, and Julia W. Snow to be instructors in botany at the University of Michigan ; F. O. Grover, of Harvard, to be instructor in botany at Oberlin College, Granville. Ohio.

Dr Fredrik Wilhelm Christian Areschoug has resigned the professorship of botany at Lund University.

Prof. E. B. Wilson, of Columbia University, has recently recovered from a serious illness, and will spend next year in travel and research abroad.

Prof. Flinders Petrie has presented to the Museum of Anatomy and Anthropology at Cambridge, nineteen cases of skulls and bones from his excavations at Hieraconopolis. These include remains of the prehistoric and earliest dynastic races in Egypt.

Lord Walsingham, High Steward of the University of Cambridge, has offered a second medal, in bronze, for specially meritorious essays in biology which do not obtain the Walsingham gold medal.

The University of Sydney is to be affiliated to that of Cambridge, and students in arts or science who have pursued a certain course at Sydney will be admitted to the usual privileges of affiliated students.

During the absence of W. H. R. Rivers with Prof. Haddon's Expedition, the course in experimental psychology at University College, London, is being directed by Mr E. T. Dickson.

Prof. J. W. Traill is to be director of the Cruickshank Botanical Garden at Aberdeen University.

Mr Briggs S. Cunningham, of Cincinnati, has given $60,000 towards the erection of a building for biology and physics at Cincinnati University.

We learn from *Science* that the U.S. Fish Commissioner has presented Cornell University with a collection of fresh-water and salt-water fishes, numbering between four and five hundred thousand specimens. The collection, in so far as

it consists of living fishes, will be of great value not only to the zoological department, but also to the College of Forestry, in which a course in pisciculture and venery is to be introduced. Duplicates of this collection are to be presented to other institutions.

Provost Harrison, of the University of Pennsylvania, has been elected president of the Wistar Institute of Anatomy, in succession to the late Dr William Pepper.

The British Institute of Preventive Medicine has recently assigned a large laboratory at Chelsea to research and teaching in technical bacteriology ; it will be named the Hansen Laboratory and be under the direction of Dr G. Harris Morris. The formal opening of the Institute will take place early next year.

According to *Science*, the University of Pennsylvania and the Academy of Natural Sciences have received from Alaska nearly 13,000 specimens, secured near Point Barrow by an expedition under the management of E. A. M'Ilhenny of Louisiana, fitted out and conducted by N. G. Buxton of Ohio and W. E. Snyder of Wisconsin. The zoological and botanical specimens go to the Academy, the ethnological to the University.

Mr Alan Owston of Yokohama has recently sent to this country a magnificent collection of hexactinellid sponges from the seas of Japan. Most of these have been purchased by the Trustees of the British Museum, but a fair number have gone to Oxford. Among the specimens are many studied by Professor Ijima for the monograph that he is writing on the group.

Mr Michael Lakin's donation of a large Liassic *Ichthyosaurus* to the British Museum, already announced in these pages, has necessitated a considerable rearrangement of the existing collection. We understand that the old cases are to be removed, while the fine slabs containing these fossils will be simply covered with glass and exhibited upon the wall. Space is to be gained by raising a number of the specimens above the top of the present wall-cases.

Dr Jonathan Hutchinson, whose educational museum at Haslemere is well known, is starting a similar establishment at his native town of Selby in Yorkshire. The building has already made considerable progress.

On October 5, a new natural history museum was opened at King Williams Town, Cape Colony.

The U.S. Department of Agriculture has sent Mr M. A. Carleton to Russia to study cereals.

The New York Botanical Gardens makes rapid progress. The museum building is complete up to the second storey, the skeleton of the whole being in place. The planting of the border will, says *Science*, be completed during the autumn. This will be about two miles long, and will contain some three hundred and fifty varieties of trees and shrubs.

A fifth International Congress of Hydrology, Chinatology, and Geology was held at Lüttich from September 25 to October 3.

The Sixth International Otological Congress will be held in London at the Hall of the Royal Colleges of Physicians and Surgeons, from August 8th to 12th of next year. The last meeting of the Congress was held three years ago at Florence, under the presidency of Professor Grazzi.

The International Conference on the Bibliography of Scientific Literature met at Burlington House, London, in the rooms of the Society of Antiquaries on October 11-13. It was attended by the following delegates :—Austria, Professors L. Boltzmann and E. Weiss ; Belgium, Chevalier Descamps and Messrs P. Otlet and H. La Fontaine ; France, Prof. G. Darboux, Dr J. Deniker, and Mr E. Mascart ; Germany, Prof. Klein of Göttingen ; Hungary, Drs A. Heller and

T. Duka ; Japan, Prof. E. Yamaguchi ; Mexico, Señor Don Francisco del Paso y Troncoso ; Netherlands, Prof. D. J. Korteweg ; Norway, Dr J. Brunchorst ; Sweden, Dr E. W. Dahlgren ; Switzerland, Drs J. H. Graf and J. Bernoulli ; United Kingdom, Sir John Gorst, Professors M. Foster, A. W. Rücker, and H. Armstrong, Sir Norman Lockyer and Dr L. Mond ; United States, Dr C. Adler ; Cape Colony, Mr R. Trimen ; India, Lieut.-General Sir R. Strachey and Dr W. T. Blanford ; Natal, Sir Walter Peace ; New Zealand, Hon. W. P. Reeves ; Queensland, Hon. Sir Horace Tozer. The chief decisions were : that the catalogue be published in the double form of cards and books ; that geography be limited to mathematical and physical, excluding political and general geography ; that a separate schedule be provided for each of the following branches of science,—Mathematics, Astronomy, Meteorology, Physics, Crystallography, Chemistry, Mineralogy, Geology (including Petrology), Geography, Palaeontology, Anatomy, Zoology, Botany, Physiology, (including Pharmacology and Experimental Pathology), Bacteriology, Psychology, Anthropology ; that Italian should be added to the list of languages not requiring translation, the others being English, French, German, and Latin. The following were appointed a Provisional International Committee : — Professors Armstrong, Descamps, Foster, Poincare, Rücker, Waldeyer, and Weiss, and Dr S. P. Langley ; the delegates were requested to organise local committees in their respective countries, to discuss matters connected with the catalogue, and to report within six months to the committee just mentioned ; and this committee is to frame a report not later than July 31, 1899. As regards future working, there is to be a Central Bureau with a Director, and as many Regional Bureaux as can be persuaded to act ; these are the workers. But there are to be International Conventions held in 1905, 1910, and every tenth year afterwards, to reconsider and, if necessary, revise the rules now drawn up. There is also to be an International Council, which shall meet in London once in three years at least ; this is to be the supreme authority over the Central Bureau. It is recommended that the International Council shall appoint for each science included in the catalogue five persons to form an International Committee of Referees, so far as possible representative of the constituent regions. It shall be the duty of the director of the Central Bureau to consult the appropriate committee or committees, by correspondence or otherwise, on all questions of classification not provided for by the catalogue regulations ; or, in cases of doubt, as to the meaning of those regulations. There are a good many more cumbrous rules, but those we have last quoted are about enough to wreck any scheme less difficult and less enormous than the present one. It is to be hoped that the director will not do his "duty," but go ahead.

THE Royal Society has awarded its medals as follows :—Copley Medal, Sir William Huggins ; Royal Medals, Rev. John Kerr and Mr Walter Gardiner ; Rumford Medal, Prof. Oliver Lodge ; Davy Medal, Prof. Johannes Wislicenus ; Darwin Medal, Prof. Karl Pearson.

GENERAL regret is felt that Sir John Evans should have found it necessary to resign the treasurership of the Royal Society. Regret has also been expressed that the Council should not have found a more eminent man of science than Mr A. B. Kempe, the mathematician, to succeed him.

THE Mineralogical Society, at its anniversary meeting on November 15, elected the following officers for the ensuing year :—Prof. A. H. Church, president ; Prof. G. D. Liveing and Dr Hugo Müller, vice-presidents.

THE late Mrs Stainton has bequeathed to the Entomological Society, London, such entomological works from her husband's library as were not already in its possession. Many of these are old and scarce.

The Geologists' Association, London, opened its session on Friday, November 4th, with a remarkably successful conversazione in the Library at University College. The following were among the numerous exhibits :—Specimens illustrating the artificial production of the structure of gneissose rocks by the deformation of heterogeneous masses of clay, and photographs of similar structures in the gneissose rocks of the Lizard Peninsula ; by the president, J. J. H. Teall. A fine series of Russian and Indian ammonites ; by Prof. J. F. Blake. Specimens and micro-sections of rocks collected in Russia during the visit of the International Congress ; by the secretary of the Association, P. Emary. Illustrations of the modes of occurrence of magnetic iron ore at Gellivara and other localities in Lapland ; by D. A. Louis. Apatite and associated rocks from Canada ; by Rev. Prof. T. G. Bonney. Granites and gneisses from the Vosges and Switzerland ; by Miss C. A. Raisin. A beautiful set of photomicrographs of recent and fossil foraminifera ; by H. W. Burrows and R. Holland. Collections of pebbles illustrating the constitution and sources of origin of high-level gravels in S. and E. of England ; by A. E. Salter. Remains of mammoth, red deer and *Bos primigenius* dredged from the Dogger Bank, as well as other interesting specimens ; by W. F. Gwinnell. A remarkable series of palaeolithic implements from West Wickham, Kent ; by A. S. Kennard. Skiagrams of fossil starfish from the Devonian of Germany, said to show structures invisible on the surface of the stone, but the originals were not brought for comparison ; by Upfield Green. Specimens, drawings and plaster casts of *Petalocrinus*, the paddle-armed Silurian crinoid ; by F. A. Bather. A fine siluroid fish from late Tertiary oil-shales of Taubaté in the Province of São Paulo, Brazil ; by J. N. Tervet. Jaws of a new dinosaur found by J. David in the Rhaetic of Glamorganshire, which jaws, we are told, will speedily chaw up certain guesses of an eminent palaeoherpetologist ; by E. T. Newton. Sir Archibald Geikie exhibited some of the new survey maps, in which many improvements have been made possible by the improved topography of the new ordnance maps ; also beautiful collotype plates which will illustrate the forthcoming memoir on the Silurian rocks of Scotland, by Peach and Horne, with petrographical notes by Teall. Many other exhibits and an enthusiastic crowd bore witness to the energy of this useful Association, and of its secretary, Mr Emary, whose address is 12 Alwyne Square, Canonbury, London, N.

The Society for the Protection of Birds announces that it has now a number of beautiful lantern-slides ready to lend to any of its members who may be able to arrange for an illustrated lecture on birds and their protection. Application for these should be made direct to the Secretary, 326 High Holborn, London, W.C.

The Report and Transactions of the South Eastern Union of Scientific Societies containing the proceedings at the Third Annual Congress held at Croydon, June 2nd, 3rd and 4th 1898, is to hand. It contains :—Places of meeting and presidents, officers for 1898-9, rules, list of affiliated societies with names of delegates, secretaries, &c., report of delegates' meeting, secretary's report, balance sheet, photographic secretary's report, lantern-slide scheme, postal magazine club, reception and exhibits, botanical research committee, referees, annual address, and the various papers read before the Congress. We have already given the titles of these, and have published two of them ; the paper on Dene-holes, by C. Dawson, has also been published in the *Geological Magazine*. This should surely have been mentioned in the report. Here, however, Mr Dawson's paper on Natural Gas is embellished by two illustrations of the Heathfield flame, one of which appeared in *Black and White*. The photographic secretary reports that a set of seventy-eight lantern-slides illustrating the Greensand and Gault has been got together, and may be borrowed by members, along with a written lecture on the subject by H. E. Turner. It is proposed to form new sets dealing with (i) Prehistoric man in S.E. England ; (ii) English wildflowers, with special reference

to forms of capsules and their dehiscences ; (iii) Coast erosion in S.E. England ; (iv) Marine organisms as transparent lantern-slides. Those willing to help this excellent project should communicate with Mr H. E. Turner, 2 Bouverie Road West, Folkestone. Another proposal is the formation of a postal magazine club, whereby members will doubtless be made acquainted with at least the titles of many magazines, but which will probably tend to lower the circulation of some publications yet further. It is an example we scarcely wish to see followed. The next Congress will be held in Rochester early in June 1899 under the presidency of W. Whitaker.

The Hull Scientific and Field Naturalists' Club opened its winter session on October 12, with a paper by F. W. Fierke, its treasurer, on "The ancient meres of Holderness and their Contents." Subsequent papers have been by J. Hollingworth on "Public Health," by Rev. H. P. Slade on "Our Water Supply." Among forthcoming items we note J. R. Boyle on "The organisation of an English Manor," and J. J. Marshall on "The Mosses of the East Riding." It is satisfactory to see how many papers are careful studies of local subjects rather than the generalities so dear to many societies of the kind. The president is R. H. Philip ; the secretary, T. Sheppard, 78 Sherburn Street, Hull.

Recent papers read before the Oxford University Junior Scientific Club have been by Dr Gustav Mann "On the Origin of Life," and by Mr A. D. Darbishire "On Natural Selection among Lepidoptera." The *Transactions* of the Club are now issued separately from the notices of meetings, and will be sent to life members post free for one year for three shillings. No. 5, recently received, contains a "Preliminary note on changes in the gland cells of *Drosera* produced by various food materials," by Lily H. Huie, and a paper on "Turpentining [*i.e.* obtaining turpentine from trees] in the Southern States [of America]," by H. E. Stapleton.

The Sheffield Literary and Philosophical Society have presented Dr H. C. Sorby with his portrait, by Mrs M. L. Waller, in celebration of his fifty years' connection with the society (1847-1897).

The British Mycological Society held its second annual meeting in Dublin, September 19-24. Excursions were made to Howth, Powerscourt, Brackenstown near Swords, the woods of Avoca, Lucan, and Dunran. These resulted in an addition of sixteen species to the fungus flora of Dublin and Wicklow ; a list will appear in the December number of the *Irish Naturalist*. Dr Plowright, president for the current year, delivered an address, discussing certain fungi figured in Cooke's "Illustrations." Papers were read by Messrs Wager, Crossland, M'Weeney, Soppitt, and Rea.

On Oct. 18, Dr John William Toore was elected president, and Dr W. J. Smyly vice-president, of the Royal College of Physicians in Ireland for the ensuing year.

The Naturalists' Society of St Petersburg has erected a new biological station on Lake Bologoy ; it is specially intended for the study of plant-plankton.

Thanks to agitation begun by the Field Naturalists' Club of Victoria, Wilson's Promontory in that Colony has been proclaimed a national park for the preservation of native fauna. This Club is also doing its best to suppress the ruthless destruction of the wattles (acacias) for the sake of their flowers. The subject has also been taken up by the Australian Natives Association.

The Scientific Alliance of New York consists of the Torrey Botanical Club, the New York Microscopical Society, the New York Section of the American Chemical Society, the New York Mineralogical Club, the American Mathematical Society, the Linnean Society of New York, and the New York Entomologi-

cal Society. It is to be hoped that some day the Alliance will have a building of its own, for at present these societies have to meet at various buildings in the city.

The fourth annual meeting of the Botanical Society of America, held at Boston, under the presidency of Dr N. L. Britton, on August 19 and 20, was most successful. Prof. L. M. Underwood was elected president for 1899. The address of Prof. J. M. Coulter, the retiring president, was on "The origin of Gymnosperms and the Seed Habit," and has already appeared in *Science*. The other papers of interest to our readers were "On Sporogenesis in *Arisaema*," by Prof. G. F. Atkinson; "Symbiotic Saprophytism," by Prof. D. T. Macdougal; "Sporogenesis in *Trillium*," by Prof. G. F. Atkinson; "Structure and development of the Centrosphere in *Corallina*," by Dr B. M. Davis; "Relations between the Forest Flora and Geological Formations in New Jersey," by Dr Arthur Hollick; "Notes on the Fertilisation of the white Pine," by Miss M. C. Fergusson; "A *Helianthus* from Long Island," by Dr N. L. Britton; "Tetradformation in Tsuga," by W. A. Murrill; and "A fossil moss from the State of Washington," by Mrs Britton and Dr Hollick.

The American Forestry Association, we learn from *Science*, held a meeting from August 23 to 25, at Boston. Reports were presented by various States as to the condition of the forestry movement. The most important feature of the meeting was the discussion of the aims and objects of the newly-established State College of Forestry at Cornell, by its director, Dr Fernow. *Science* promises to print his address in full.

The Commission appointed by the Colonial Office and the Royal Society to investigate the mode of dissemination of malaria with a view to devising means of preventing the terrible mortality which now takes place among Europeans resident in tropical and sub-tropical climates will consist of Dr C. W. Daniels, of the Colonial Medical Service, British Guiana; Dr J. W. W. Stephens, formerly Lawrence Student in Pathology and Bacteriology at St Bartholomew's Hospital; and Dr R. S. Christophers, of University College, Liverpool. Dr Daniels will proceed at first to Calcutta, where he will acquaint himself practically with the remarkable work which Surgeon-Major Ross, of the Indian Medical Service, is carrying on into the relation of mosquitos to the dissemination of malaria. Drs Stephens and Christophers will spend some time in Rome studying malaria. Subsequently the Commissioners will meet at Blantyre, British Central Africa.

A Commission—consisting of Dr Thomas R. Fraser, F.R.S., Professor of Materia Medica and Clinical Medicine at Edinburgh University (president); Dr Wright, Professor of Pathology at the Army Medical School, Netley; Dr Rüffer, who has been for some time head of the Egyptian Sanitary Department at Cairo; and two officers of the Indian Civil Service, Mr J. P. Hewett, C.I.E., and Mr A. Cumine, both of whom have had much to do with recent plague affairs in India—has been appointed to report on the following matters concerning the plague in India: (1) the origin of the different outbreaks; (2) the manner in which the disease is communicated; (3) the effects of certain prophylactic and curative serums that have been tried or recommended. The Commission reached Bombay towards the end of November.

Mr E. W. L. Holt has been appointed by the Royal Dublin Society to make researches over a period of five years on the life-history of the mackerel. A ship is being fitted up as a floating laboratory at Berehaven which will be Mr Holt's headquarters.

Hamburg has founded at Freihafen a station for the protection of plants against injurious insects introduced from abroad, and against plant-disease generally.

THE half-yearly report of the inspector for the eastern sea fisheries district just issued, states that the fishing during this season has been much above the average. The rigid enforcement of the regulation against taking undersized shell fish has benefited the fishing-grounds. The chief offenders in this respect are the lobster and whelk fishers, whose practice it has been to use undersized crabs for bait. At one place alone the destruction of undersized edible crabs for this purpose has been upwards of 3,000 in each day's fishing, and the number of 'unsizeable' crabs destroyed along the Norfolk coast has been greater than the whole number of 'sizeable' crabs landed. The wholesome effect of prosecutions and convictions, and the increased efficiency of inspection during recent years, is shown by the fact that "all the fishermen report that the ground contains more small crabs than have ever been seen by them before."

CANADA is to have a floating biological station in the Gulf of St Lawrence for at least five years, and the Government of the Dominion has appropriated £1400 for the purpose. The board of management consists of Prof. E. E. Prince, director; Professors D. P. Penhallow and E. W. MacBride, of McGill University; Prof. Ramsay Wright, of Toronto University; Prof. L. W. Bailey, of the University of New Brunswick; Prof. A. P. Knight, of Queen's University; and Rev. V. A. Huart, of Laval University. For the first year the laboratory will be on the south shore of Prince Edward Island, and will be moved annually. It is hoped that active work may begin early in 1899.

MR GEORGE MURRAY, Keeper of the Botanical Department of the British Museum, has organised an expedition for the study of intermediate ocean depths. With the aid of the Royal Geographical Society, the Drapers' Company, and the Fishmongers' Company, he chartered the ss. "Oceana," which was fitted with deep-sea gear by the Silvertown Telegraph Cable Company. Mr Murray is accompanied by two of his colleagues, J. W. Gregory and V. H. Blackman, as well as by Dr Sambon, J. E. S. Moore, and Percy Highley, the last-mentioned acting as artist. The steamer, after some delay from fogs, left the Thames on November 16, and proceeded directly to the west coast of Ireland, where work was begun at the edge of the 100-fathom platform, about thirty miles west of Dingle Bay. It was intended to steam slowly for about 10 degrees westward, making continuous observations with a vertical chain of tow-nets, which would gradually be lowered until, with a length of 2000 fathoms, the series would include 38 nets. Thus the difference between the faunas of different depths in the same part of the ocean can be estimated by comparing the contents of the nets. Experiments with various forms of self-closing nets will be made for the sake of comparison. Soundings and observations of temperatures will be taken, and there may be opportunity for some deep-sea trawling.

A DETAILED natural history survey of the long sand-bank known as the North Bull, in Dublin Bay, recently undertaken by Messrs Praeger and Halbert, is, says the *Irish Naturalist*, turning out unexpectedly interesting from a zoological point of view, as this apparently inhospitable spot has already yielded several additions to the Irish fauna.

MR JOHN S. BUDGETT has started for the Gambia, under the instructions of the Zoological Society of London, in order to gather information concerning the larger mammals of the colony, and to make zoological collections, especially of the fishes.

THE following telegram has been received from Dr J. Stadling, who was sent by the Swedish Geographical Society to search for Andrée. It is dated Irkutsk, November 2:—"There being no telegraph in Siberia north of Irkutsk, I am sending this note by messenger up the River Lena to that place for telegraphic dispatch. We are now, on September 15, at the Lena delta, having been pursuing

our quest for news of Andrée since last April. We have got our row-boat ready, and in this are now starting to cross from the Lena delta to the mouth of the Olenek. If we arrive in safety we shall from thence proceed in sledges to Chatauga, the Taimyr Peninsula, and the mouth of the Yenisei in our search. The botanist who has accompanied us thus far is now returning *via* Irkutsk."

Mr N. R. Harrington and Dr Reid Hunt have returned to New York from an expedition to the Nile Valley, where they went to obtain material for the study of the life-history of *Polypterus*. The best fishing-ground was at Mansourah, forty miles from the sea. The study of this was expected to throw light on the relations of the Crossopterygian fishes to the Amphibia and the Dipnoi; but unfortunately no embryonic material was obtained. Excellent collections were, however, made for other zoological purposes.

Mr Cornelius Vanderbilt will provide funds for a botanical expedition to Porto Rico, to be undertaken by the New York Botanical Garden, under the direction of Dr N. L. Britton. The expedition left towards the end of October, and will collect for six months.

News comes from Argentina that Dr Santiago Roth, of the museum of La Plata, has discovered a series of Mesozoic mammalian remains in the Territory of Chubut. Palaeontologists will await with great interest his promised memoir on the subject.

The report of the progress of the Ordnance Survey up to March 31, 1898, has been issued as a Parliamentary Blue-Book, which is thus summarised by *The Times*. Dealing first with England and Wales, the report states that the publication of the revision of the cadastral survey on the 1-2,500 scale is proceeding as rapidly as possible, the total area published being 9,236 square miles, of which 5,217 square miles have been published during the year. The revision has now been taken up of all the counties of England and Wales which were surveyed more than twenty years ago. The publication of the revised maps on the 6in. scale has been hitherto much retarded by the publication of the new maps on this scale of London and the Tyneside towns. This latter work is, however, now approaching completion, and more rapid progress is expected in future. The total area on this scale published is 3,788 square miles, of which 3,609 square miles have been published during the year. With regard to the revision of the 1in. map, which was sanctioned by the Treasury in 1893, the report states that the field-work of the revision was begun in 1893, since when the whole of England and Wales has been revised on the ground, with the exception of a few streets in the midland counties and North Wales, which will be completed this year. Of a total area of 58,527 square miles thus revised 14,643 have been revised during the year. The revised maps of 28,305 square miles have been engraved and published, 5,282 during the year. The general result to be obtained by the revision is that in 1899 there will be available to the public for the first time a 1in. outline map of the whole of the country, prepared on one uniform system, and with its principal details nearly up to date. It is further stated that the revision of the map in the scale of 4in. to the mile will follow on that of the 1in. map, but cannot proceed very rapidly until the latter has been completed. After giving some details with regard to Scotland and Ireland, the report, in a summary and tabular statement of progress for 1897-98, shows that, so far as the original surveys of the United Kingdom are concerned, the town surveys for Great Britain and Ireland, the 1-2,500 maps for Great Britain, the 6in. maps, and the 1in. maps for Great Britain and Ireland have all been completed, while the hill engraving for the new series 1in. map of England and Wales is proceeding, 4,262 square miles out of a total of 27,569 published on March 31, 1898, having been published in 1897-98. Considerable work has also been accomplished in the way of resurveying.

CORRESPONDENCE

THE CLASSIFICATION OF BUTTERFLIES

In brief reply to your note on page 298, vol. xiii., I would state that the parity of specialisation in the feelers of the Papilionidae and Nymphalidae does not necessarily authorise the association of the groups. I have not separated the Papilionides (Parnassiidae and Papilionidae) upon any characters of specialisation, but upon a fundamental divergence in the structure of the wings, such a divergence as indicates, in my opinion, a distinct phylogenetic line and is plainly available for taxonomic purposes. The gist of my article in *Natural Science* and of that in the *Proceedings of the American Philosophical Society* is, that parallel specialisations in any one organ do not authorise association, since they have been independently acquired. Rank is a relative conception. I do not claim that the Papilionides should outrank all the other butterflies, but I do claim that they cannot be logically interpolated between any of the groups of the other butterflies, a mode of classification as old as Roesel, revived by Bates, Distant, Scudder, Reuter, and others. This interpolation cannot take place because the Papilionides possess, in vein ix. of primaries, a morphological character unshared by all the other butterflies, which again have in common a loop-like appendix to vein vii. at base (vein viii.). This appendix fades out by disintegration in certain minor groups. It is also shared by the more specialised and larger groups of the moths. The butterflies are therefore probably dichotomous, and I expect the phylogeny of the Papilionides is to be sought for in the Tineides. The two groups of diurnals have attained their character as day fliers by different routes.

My criticism of Dr Jordan's investigations is, that, valuable as they are as a contribution to the morphology of the feelers, they are valueless for taxonomy and phylogeny. I do not regard the specialisations of the veining, which takes a parallel direction so far as concerns the radius and its branches in the Lycaenidae and Pieridae, as any warrant for the association of these families. And I must naturally object to having my classification of the butterflies into two major groups, viz., Papilionides and Hesperiades, criticised upon the basis of coincidences in specialisation of certain organs common to both groups. The Papilionides possess an important part of an organ which the other butterflies want. Specialisations come in afterwards, as when I show that the Parnassians are more specialised than the Swallowtails, and that therefore we should commence our lists with *Apollo* and kindred butterflies. A. Radcliffe Grote.

Roemer Museum, Hildesheim, *7th November* 1898.

IMPORTANT NOTICE

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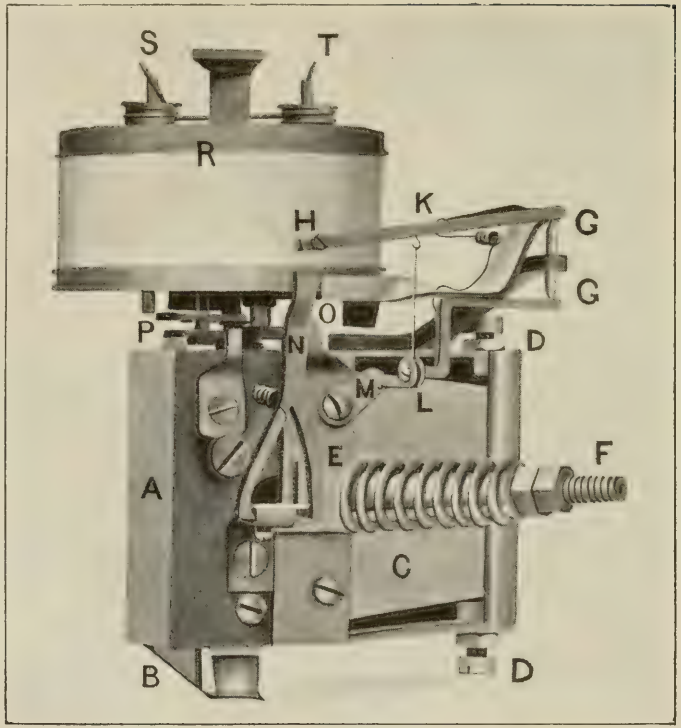
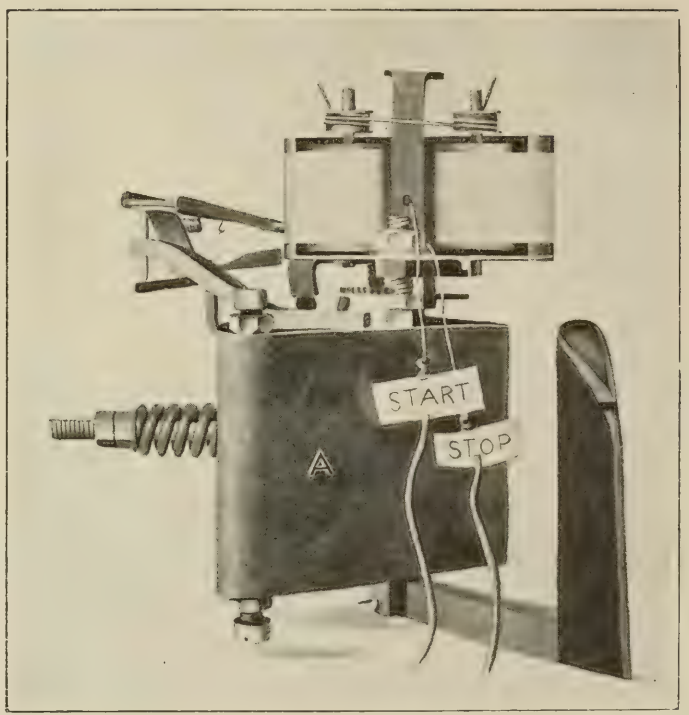


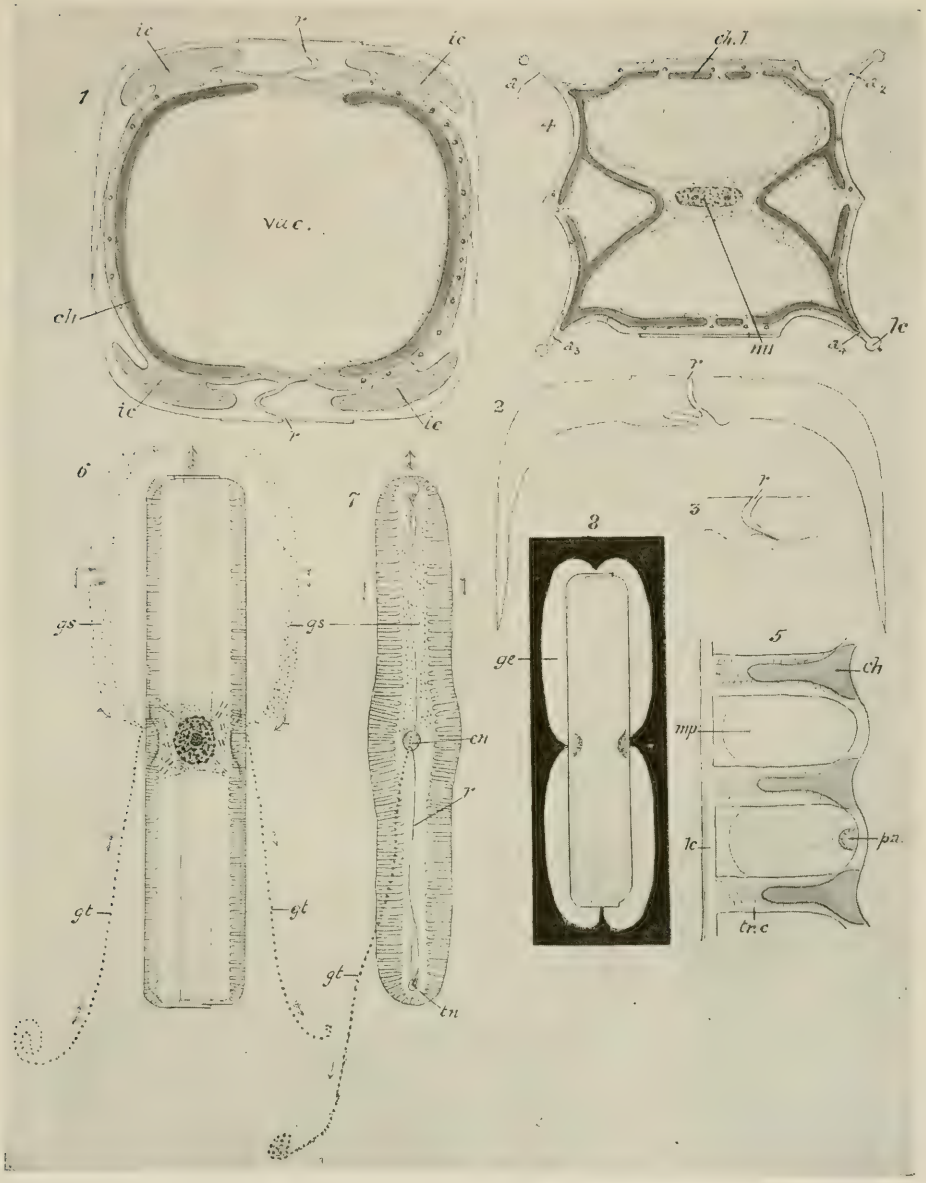
Back View.



CONTINUOUS RECORD INDICATOR, *with cover removed, to show Pencil Attachment and Drum.* *A*, Front Plate; *B*, Bottom of Axle; *C*, Link; *D*, Dead Centres for Link; *E*, Plate End of Spring; *F*, Core and Link Support of Spring; *G*, Vertical Axis of Pencil Attachment, but Horizontal Axis just above: *H*, Pencil; *K*, *L*, *M*, Chain; *L*, Pulley; *N*, Winding Lever; *O*, Winding Wheel to left of this; *P*, Cam for putting Pencil out of action; *R*, Drum on which diagram is drawn; *S*, *T*, Cylinders holding paper.

A HEDGEHOG (*Erinaceus europaeus*) WITH HAIRS INSTEAD OF SPINES

(*This block is kindly lent by Prof. A. Fritsch*)



THE STRUCTURE OF DIATOMS, AFTER LAUTERBORN.

FIG. 1.—Transverse section through *Pinnularia major* in the region between the extremity of the cell and the median mass of protoplasm (combination figure). *Ch.*, chromatophore; *ic.*, chambers ('Riefen') on inner surface of cell-wall; *r.*, raphe; *vac.*, vacuole.

FIG. 2.—Transverse section through one valve of the frustule of *P. major*. The section passes between two of the chambers, so that the contour of the cell-wall is unbroken except at *r*, the raphe.

FIG. 3.—Transverse section through the raphe, from another section, showing an apparent closure of the cleft internally.

FIG. 4.—Median transverse section through *Surirella calcarata* (combination figure). At a_1 the section passes through one of the intermediate pieces in the ala, at a_2, it just encroaches upon one of the transverse canals; a_3, a_4, two transverse canals bisected; *chl.*, superficial lobular process of chromatophore; *lc.*, longitudinal canal; *nu.*, nucleus.

FIG. 5.—Surface view of a portion of one of the alae of *S. calcarata*. *Ch.*, process of chromatophore surrounded by protoplasm; *lc.*, longitudinal canal; *mp.*, intermediate piece; *pa.*, unicellular alga; *trc.*, transverse canal.

FIG. 6.—Lateral view of *Pinnularia major* moving in an emulsion of Indian Ink.

FIG. 7.—Surface view of the same. *Cn.*, central node; *gs.*, anterior granule streams; *gt.*, gelatinous threads with adhering granules; *r.*, raphe; *tn.*, terminal node.

FIG. 8.—Resting example of *Pinnularia* in concentrated emulsion of Indian ink (lateral view); *ge.*, gelatinous envelope.

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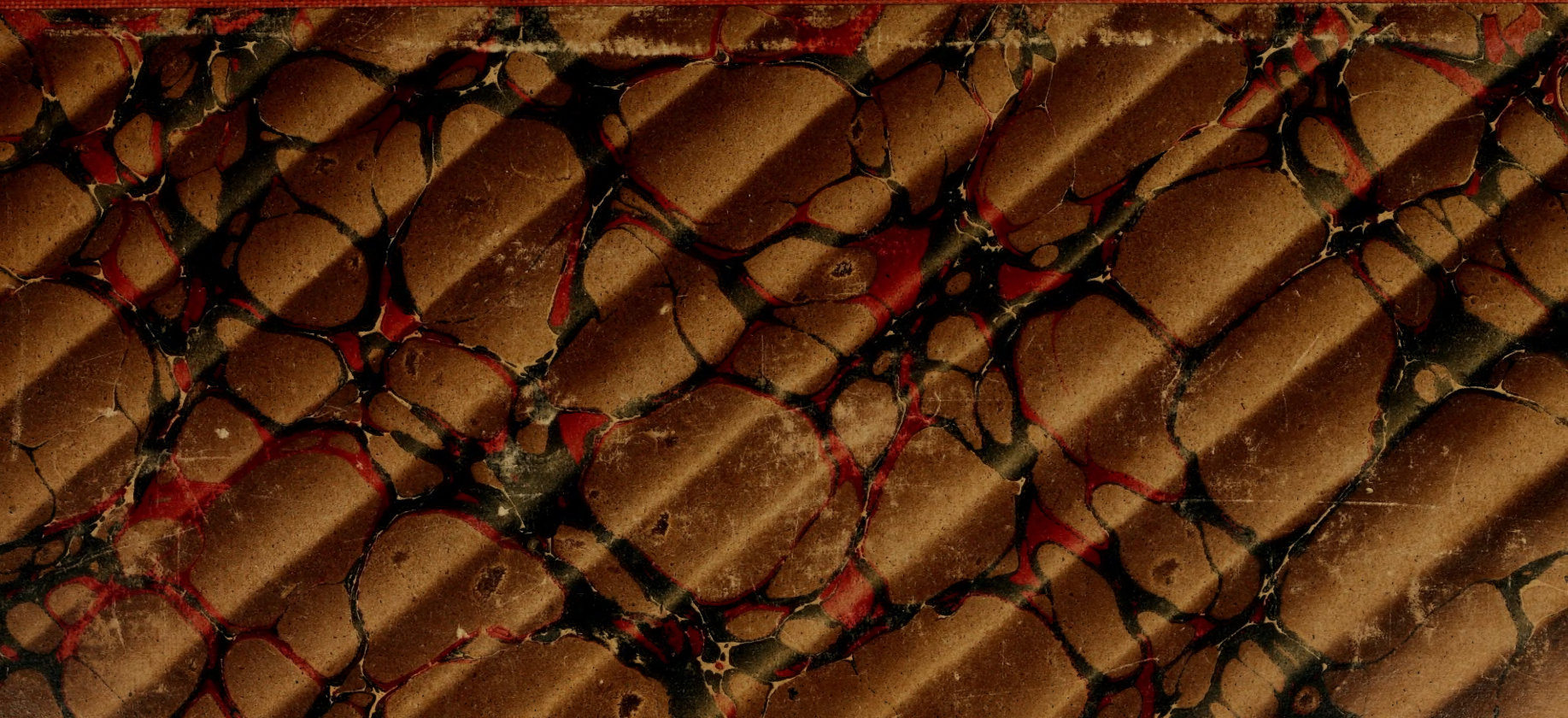
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